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THE TREATMENT OF SEWAGE.

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From the "Journal of the Society of Arts."

When, some six months ago, your Council did me the honor to ask me to read a paper before your Society on the subject of the "Treatment of Sewage," I accepted with considerable misgivings, knowing that to do the subject in any way justice would necessitate an amount of work which I was not quite sure that I had time for at my disposal. However, I accepted the duty, and I at once set to work to think out the whole matter. I had a vast amount of facts to go over, and the result has been that I have prepared a paper which it is impossible to read for two reasons—first, its length, for I think it would take almost a week to read, and, secondly, that it is not in proper form, which is a very important matter in connection with this subject. I only mention this in order to assure the Society that I have not neglected the subject which I have been asked to bring forward. Perhaps I have done a little too much, so far as that is concerned, in its preparation; and I shall have to ask you to forgive me if, instead of reading the paper, I give an outline of the conclusions at which I have arrived, and of certain facts bearing on those conclusions, and, at the same time, if I avoid, as far as possible, anything like details. These will all be printed in due course, and then the details upon which I have based my conclusions will be before the Society.

I should like to mention one or two other preliminary points. First of all, I have tried to consider this subject unfettered, as far as possible, by previous conclusions. I remember that famous saying of one we know very well, Prof. Clifford, that consistency was the bugbear of weak minds. I have not tried to be consistent. I have attempted, at any rate, to approach the subject from the position of judge, if possible, and not from the position of advocate. I come before you without any patent; I am an advocate of no system. I have been retained by a good many people on both sides in my time, and I have had an opportunity of knowing what is going on behind the scenes, and, as I say, I come here to-night without being an advocate of anybody, or anything, without any patent to support, and without, as far as I know, having written anything upon the subject which can be criticised. So that, I think I am in a perfectly independent position in this matter. Another matter which I should like to mention is my obligation to many friends in preparing the paper, and I mention that so that it may not be supposed I am taking upon myself credit for work which belongs to others. First of all, I must mention my friend, Professor Dewar, whose originality we all know. Then there are others, earnest, ardent precipitationists, like Mr. Hawksley. I

cannot mention all their names at every turn, and therefore I must, once for all, admit the obligations which I owe to them before I start.

Now, I have endeavored to come to something like a conclusion as to the proportions of solids and liquids in sewage which are contributed daily by a town population of a given number. And I have endeavored to do this as far as possible by calculations. Those calculations have been made to a great extent at the London Hospital, where we have something like 900 people, and where we have, therefore, a very good opportunity of judging. I am not going through the actual numbers to-night, but I may say that if you take a town population of 10,000 people, you will not be very far wrong in saying that the urine of that 10,000 of town population will be something like 10,300 gallons, of which from 740 to 750 lbs. is organic matter, and of that organic matter it may be taken that about 220 lbs. is nitrogen. Then, as regards the fecal matter, it may be taken that the dry fecal matter from the 10,000 town population will be about 430 lbs. There will be that quantity of dry fecal matter passed per day by that number, and of that quantity about 360 lbs. is organic matter; of that 360 lbs., something like 23½ lbs. is nitrogen. So far as I can make out, that is as nearly as possible the state of facts.

Now, then, of the constituents of this organic matter, the phosphoric acid and potash have a certain manurial value. This annual value of the excreta has been calculated by a great many different people. I do not know myself very much about the agricultural aspect of the subject, and I have therefore looked up all the estimates of agricultural writers bearing upon it. It is very difficult to make anything of them, for they vary from 6s. 6d. to £1 as the annual value of the excreta of each adult. I think you may take it that between 8s. and 9s. is more nearly the actual result than any other I can arrive at. Then we have got all sorts of calculations about the ammonia. We are told that urine, on the average, contains 10.3 per cent. My own experiments leads me to think that that is rather a low estimate of the actual quantity—decidedly low I think—when you take into consideration the fact that a large number of the population of towns are, generally, not in very good health, and that the amount of ammonia excreted during disease is not at all the same, but very often double what it is during health; however, that does not make very much difference. Then the value of the ammonia alone is something between 6s. 8d. and 7s. 6d. per year for each adult. All I want to make clear is—for this is the point I wish to arrive at in connection with this question—that the chief subject of manurial value is urine. It is very curious to see all the estimates that have been made of the relative valuations of this excreta. We have, for instance, the statement that a pound of human excrement is equal in manurial value to 13 lbs. of horse dung, or 6 lbs. of cow dung. A very famous chemist has said that the excretal value of one adult is equal in manurial value to the droppings from one sheep, and so on. I can only mention these facts to you as an introduction to my story.

Well, the first calculations with regard to the value and quantities of this excreta were made from the cesspool system and the midden system. The middens were not looked upon as sources of profit, but simply as means to prevent nuisance. Cesspools had many disadvantages; there were noxious emanations from them, polluted wells, and all sorts of things. The middens have their advantages, and they also have undoubtedly their difficulties. One of the difficulties in connection with them was that of educating the people to use them properly; a difficulty which, in my opinion, applies just as much to water-closets as to middens. Another difficulty was to get the local authorities to attend them properly, I mean with regard to the perfection of the scavenging arrangements; and I am bound to say that, in my experience, the difficulty is not one iota less in getting the local authorities to treat the sewage properly. The middens have two advantages; the first is the diversion of excremental matters from the rivers, and the second, that disinfectants can be used in middens during periods of epidemics. Those are very great advantages which we have to consider in connection with this subject.

Now, there are two matters which I have to mention with regard to a circum-

stance which was pointed out by Dr. Frankland, which is that sewage from midden towns is just as foul, just as impure, and just as polluting, as the sewage from water-closet towns. And Dr. Frankland, in one of the reports of the Rivers Pollution Commissioners (for, of course, although it was the report of the Commissioners, it was really his report), has given figures to show that the sewage from these midden towns is as bad as that from water-closet towns. I emphatically join issue with him there on the facts. My own experience is that he is absolutely wrong somewhere. I do not know exactly where, but I am perfectly clear that he is wrong, and that midden town sewage is not so bad as water-closet town sewage. And it is a very curious fact also, that it is very clear from Dr. Frankland's own results that his results show he is wrong somewhere, because he has taken this sewage of which he has given the composition from a large number of midden towns; and after giving the details showing that the sewage of midden towns is the same, or rather worse, than the sewage of water-closet towns, he says that the facts also show that an average of 25,561 tons of solid matter is annually kept out of the midden towns' sewage which has been examined. Well, I cannot make out exactly how, having come to the conclusion by elaborate calculations that out of the midden town's sewage which he has examined there is kept out 25,561 tons of solid sewage matter, he can say that the sewage is as bad in the one case as in the others. That is one of many difficulties which I have found at starting. My own calculations show that Dr. Frankland is correct in the quantity of solid matter which is kept out of the sewers, and by that quantity of course is the sewage of the midden towns improved. That is the conclusion at which I have arrived on that point; therefore, I join issue with him, and it is very important that I should join issue with him there, because it is a matter on which I shall have to speak. It is no use saying that middens, if not properly looked after, are a nuisance; of course, anything that is badly looked after is a nuisance. I only say that it is important that we should notice those facts.

It is impossible that I should go here into anything like a discussion of the merits of the dry-earth system. As regards cost, I will simply say this: that two men and one horse can, at any place where the place of collection is ordinarily near a town, collect three pails a week per head at the cost of 3s. 9d. per ton. That seems to be about the result.

And now we come to the water-carriage system. People said, "Here is this horrible stuff to be got rid of; what can there be better to get rid of it than water—wash it away as fast as possible." That was the idea—thus arose the water-carriage system.

And what is the value of the sewage thus produced? Sewage liquid, compound—very compound; complex—very complex. We may say broadly, if we want to define sewage (and I have defined it on many occasions) that it is the refuse of communities, their habitations, streets, and factories. This is not a very accurate definition, but it is somewhere about it. Now, then, as regards the quantity of sewage and the quality of sewage, I think there are two facts to be borne in mind. I have the analyses of sewage from a great many different towns which have come before me, but there are two facts which I want to bring before you, and the first is, that the quantity of sewage and the quality of sewage in one town is entirely different from the quantity and quality of the sewage in another town; it is totally different, varying with different conditions, such as the trade operations that are being carried on there. I want to show that there is a difference from that fact alone. That is a difficulty, and when we hear a man saying that this is the system of treatment or that is the system of treatment, we must remember that the sewage of different towns differ altogether. There is something else, because, secondly, I say that the sewage of some towns varies at different times of the day, and under different conditions of weather. It is no good your going down to a town at a particular time and taking a sample, and saying, "This is a sample of the sewage of the town." As a matter of fact, it is not a sample of the sewage of the town; it is only a sample of the sewage of the town which has been collected at the outfall at a particular time, and nothing else; and I think it is im-

portant, therefore, on this question of the treatment of sewage, that we should distinctly bear that fact in mind. As I say, the sewage of one town is not at all the same as the sewage of another, and the sewage of the same town even varies at different times.

Now, I am not going to discuss here any question of the advantage or otherwise of the separate system. I do not want to enter into it to-night, though I have discussed it fully in the paper I have prepared. I have no doubt of this, that for the purposes of irrigation separation is an advantage. For the purpose of precipitation, dilution, within certain limits, is not an evil, and we must remember that the question of the separate system must bring before us the question of what you are going to do with the sewage—what sort of system you are going to adopt for the purpose of getting rid of it. We must remember this, a steady uniformity of views is, no doubt, a very good thing, but a steady uniformity of views is not everything. Steady uniformity is at times a perfect nuisance; in fact, there is a great evil in having steady uniformity in everything, and especially in sewage matters. You lose the advantages in this case of a good flushing; and I do not know anything that is a greater loss than that. It is very easy to be very clever on the separate system, but I venture to say that the separate system would mean an intermittent pollution of all the watercourses of the country. There cannot be a shadow of a doubt that the foulest sewage is brought down in the first hour's rain from the washings of the streets and such like. I am afraid that, in these days, we are getting to be too much phrase worshipers. We all know the phrase about the "Rainfall to the river and the sewage to the soil." It is very pretty, but it is not correct It is all very well throwing this phrase at you, using it as an argument, but it is no argument at all.

Well, I come now to the composition of the sewage. I am sorry to say I cannot give it you exactly. You cannot tell what is the composition of sewage. It varies in a number of different ways. Suppose, for instance, I take the average of an enormous number of sewages, and say we get about 45 grains in solution and about 44 grains in suspension, I should not be very far out; but at the same time that is not a very satisfactory statement after all, because the composition of sewage differs at ever hour in the day. There are weak sewages and strong sewages, average sewages and non-average sewages. We have all sorts of sewages. The term "composition of sewage" is, therefore, simply nonsense. We cannot speak of average compositions unless you take samples of the sewage every half-hour, and then mix all the samples together, and make an analysis of the mixture of the half-hour samples during the twenty-four hours. That gives you an idea of it. But the idea of going and taking a sample merely, and saying that is a real example of the sewage of a town, is nonsense. Well, then, the great thing of course to be done was to get rid of the sewage; and the water-carriage system was introduced in order to wash it away from your houses. But, then, the next point was how to get it away from you, how to get rid of it as soon as possible; and of course the easiest way to do that was thought to be to send it into your rivers. All I can say is, that letting it go into your rivers means, for the most part, putting off the nuisance from yourselves to your neighbors. Of course it is a very easy way of doing it, but the great evil of letting your sewage go into your rivers is from the suspended matters which you have in your sewage. I do not think the other part of it makes much difference. The matters in solution do not make much difference if they are well mixed, because they soon got oxydised. Everyone believes that, I think, except one or two individuals, nowadays, and I will say no more upon it. Then with regard to the solid matters, we generally find that the solid matter flows about the outfall; but some of the matters in solution combine with the aluminous constituents from alluvial soil, and so forth, and become insoluble, and then we get a curious sort of suspended matter. We find, for instance, the manurial matters going down first, and then, as the mixture of sewage and water flows on all the organic matters, of varying density, go down. Of course those that are lightest or most flocculent of all go down last, and very often that is the most offensive

of all kinds of organic matter; so that when you let your sewage matter go into your rivers, you do not, perhaps, get much nuisance near you, because, of course, the organic matter has gone down lower. The organic matter sinks to the bottom of the water, and there undergoes putrefactive decomposition, until after a short time the gases developed are sufficient to render this material specifically lighter than the water; then it comes up, gives off its gases, becomes heavier, goes down again to putrefy once more, with other matters added to it, then comes up again, and gives off other gases. That is the history of suspended matter introduced into the river in that way.

Well, it was quite clear that would not do. People could not stand that sort of thing, and so they set to work to see what they could do with their sewage in some other way. Two things could be done with it; but let me put one first. It was absolutely necessary that one thing should be done with it; that is to say, that it should be purified, that was certain. And the second thing was that, inasmuch as the sewage had a certain manurial value, it was desirable to utilise whatever valuable constituents were present in it. Those were the two conditions required. First, to get what you could out of it, and next, to make it as pure as possible. But there was something else. Of course you must purify it, or else there would be litigation, and therefore you must do that, and you ought also to do your best to get all you could out of it; but whatever you did with it, you must do it without causing a nuisance. Those were the conditions, that you must purify it, and that you must get what you could out of it; but you must do both without committing a nuisance.

Now comes the question of the value of sewage. It is wonderful how people talk, or rather how they used to talk—they do not talk quite so much about the value of sewage now. The basis of the calculation is simplicity itself. There can be no doubt we can easily calculate the value of sewage. Dr. Corfield does it in his book with the greatest ease in the world; you get your population, you find the quantity of the excreta, you find out the constituents and the relation of one to the other, and there is your calculation. Well, it was very curious that a calculation so simple as that should have brought forward a heap of authorities who differed very widely as to what the value of the sewage was. I have just been looking through the evidence which was given before the Select Committee of the House of Commons in 1862, and there the authorities differed between a halfpenny and 9d. a ton. It was very curious, seeing the simplicity of this little calculation, that they should differ so much as that. They said there was nothing easier than to calculate the value of your sewage, and yet they differed so widly as that—between a halfpenny and 9d. The interest, of course, of this whole matter is centered in the value of the sewage of London; and I have been trying to get together the views of all the people who have views on the subject, for the purpose of seeing what I can make of it. There is a class, which I will call the cautious people, who, taking the population as 3,000,000, estimated the value at 6s. 8d. per head, and they calculated the value of the sewage of London as £1,000,000 a year. Then there was another class of less cautious people, Mr. Brady and Lord Robert Montagu I will put amongst them, who thought that to take 6s. 8d. was nonsense, and they made out the value of the sewage of London as £2,890,000. Then there was a class of very heroic people, as I will call them, numbering amongst them eminent chemists, and they calculated the value of the sewage of London as £4,081,430. Those are the different opinions which have been given about the value of the sewage of London. When you asked them about it, they said, "Our calculations are based on the teaching of science." I am carefully using their own words, that they were "based on the teaching of science." Well, of course, if this sewage is of this extraordinary value, we must try and get something out of it to help to pay the rates, and people said—let us apply it to London. That naturally resulted. Enthusiasts fully believed in its value, and arithmeticians and scientific men were found to start sewage farms. I see before me a very distinguished man, who, I think, was one of the earliest of those who tried the plan, but a certain unpleasant awakening occurred with regard

to all this. Science told its story, and through the mouths of its authorities, some of whom I see before me, told that story well. They told us how the land acted,—how it was a mechanical filter, and a chemical laboratory at the same moment; how as a mechanical filter it took out the solid matter; how as a chemical laboratory it effected the oxidation of the nitrogen which the plants took up and utilised, and brought back again for our use as food. They told us how the churchyard feeds the village sheep; how the sheep feed the villagers, and how the villagers feed the churchyard herbage. They were always telling stories of this kind. Science talked in this way, but then the farmers had something to say about it. They said, "All that is true, no doubt; your learned societies say so, and, therefore, it must be true."

Still the farmers had their own views on the subject, and their views led to this conclusion, that they would only take the sewage at such times as they liked, and in such quantities as they required. What is the result? Some great and learned people thought the farmer very foolish, very ignorant, and very obstinate. I do not think I am putting that too high. They traced the obstinacy of the farmer to his conservatism—I mean his blind attachment to old-fashioned ways, good and bad. The facts did not support this view, still they stuck to the fact that the folly of the farmer was his conservatism. Well, the farmer does not quite follow this conservatism in everything; he shows his appreciation of newly invented manures; he thoroughly understands the value of superphosphate. He is not conservative in that. He is not conservative in adopting new agricultural implements; he does not show any blind attachment to old fashioned ways there, but he did in this matter of sewage. Mr. Chadwick preached with the authority of a Board of Health at his back. I will quote his own words, so that there shall be no mistake about it:—"That liquid manure was at all times preferable to solid manure, and suitable for all crops, and all soils;" but Mr. Chadwick's sermon fell upon bad ground. Mr. Chadwick piped, and the farmers refused to dance. Seeing that this part of the subject was a failure, gentlemen farmers set to work. They set to work for the purpose of teaching the farmers, but after a while even the gentlemen farmers gave it up. They learnt a lesson at some cost, and left off their work, sadder, wiser, and poorer men. Mr. Chadwick still preached, but the people began to suspect one or two things, either that there was some obstacle to the agricultural use of sewage, or that its theoretical was not its practical value. Local authorities too found this, that if they had to apply sewage to land in order to purify their sewage they must secure the land, they could not trust to the farmers taking it. Why? Now comes the part of my story that is important. Why did they require land? First, that the sewage might be taken at all times, day and night, Sundays and weekdays, all the year round, summer and winter, whether the soil wants it or not, or there are any crops or not, and at all stages of their growth, seed time and harvest. The farmers did not see the force of that. Secondly, they found out that it was necessary so to purify the sewage as to produce an effluent which, under every consideration, should not produce a nuisance, nor pollute a watercourse. The farmer looked after his crops; the local authorities looked after their effluent, and the two things did not agree. The farmer said, "I admit a certain manurial value; I am not going to dispute that science teaches it; I should not care to discuss the question with science; but on the whole, having given the subject my consideration, I prefer to sacrifice the manurial value rather than be compelled always to have the water." That was the conclusion of the farmer.

Besides, he says, "it is no use your pointing to other countries, and telling me what a wonderful thing irrigation is in India, and in Continental places, and so on, that will not do; temperature is against us here; wet weather is against us; look at frost" (and I shall say something about that before long); "allow your ground to remain quiet and aerate it, which was necessary, and the ground freezes; neglect to aerate it, and it is no good." That is the first great difficulty. It is no good telling me about the increased temperature of sewage, and I

shall have something to say about that before long. Frost, rain, and storm, water-logged land—the very time when you have the most sewage to deal with, the ground is in the worst condition to take it. Hot weather cracks your clay, if you have clay, and down goes the sewage without being purified at all.

Dr. Carpenter, who is an ardent irrigationist, has pointed out the difficulty of management, and there are some remarkable passages in Dr. Carpenter's paper, when he was, perhaps, forgetting himself a little on that point. Increased cost — the Local Government Board know something about that in their report. Then we come to this curious fact, that you cannot get one single point upon which these ardent authorities are agreed. I have been trying to see if we can get a starting point on which they are agreed. They do not agree upon the method of application—whether it shall be by hose and jet or not. "The soil best suited is very porous soil," said Professor Way—and he knows a good deal about it; "a heavy soil," says Liebig—and he knows a good deal about it; and then others go between the two. Then some say that the crop most suited is rye-grass, and I think they are right. Others hold different opinions.

Now comes a crucial question—How much sewage *qua* agricultural success, can properly and, *qua* sanitary success, safely, be applied to a given area of land? Now I will ask you for a few minutes to regard this question of area before 1870 and after 1870, and, I was almost going to add, after April 14th, 1886; but I will take it, if you please, before 1870. The agricultural and the sanitary aspects of the subjects were not in accord. To realize your agricultural success you must apply a large quantity of sewage to your land, and that you know they did. Realize your sanitary success, and then the rule is, apply a smaller quantity, the smaller the better. So you have for agricultural success to get as much as possible out of sewage; and for sanitary success, in order to purify it to have as little as possible. Can you split the difference? The authorities differ between 2 and 800 as to percentage. Mr. Mechi said 2, but he altered his views afterwards; Mr. Hope said 20 or 30; Mr. Way and Sir Robert Rawlinson 100 consistently; and I think I shall not be unfair if I state it thus, that the authorities on the subject all say (and recollect I am speaking of before 1870) that 100 is the minimum to pay, and then a great many other persons said that 30 was the maximum to escape prosecution.

Our Friend Dr. Frankland saw that difficulty. He saw what I will call the land difficulty; he saw this, that the greater the quantity of sewage that you have the more land you required, and the less land there was to have. For a very large town there was very little land in its neighborhood. A small town had plenty of land, but there was very little sewage. Not only did you require more land where you had more sewage, but the higher was its price. Dr. Frankland grasped that difficulty, and he said to himself, I have no doubt irrigation is doomed, unless I can find out some method of concentrating a large volume of sewage on a small area of land. Well, Dr. Frankland in this, as he has done in a good many other difficulties, saw a way of escape. In the year 1870, Dr. Frankland set to work to make a series of laboratory experiments. He took a cube foot of earth, and calculated the amount of sewage that that cube foot of earth in his laboratory would purify. I am very particular about this, because a certain letter in the *Times* of to-day, about which I shall have something to say hereafter, necessitates my drawing attention to this. I am sorry Dr. Frankland is not present to hear the criticism which I have to offer; but I know he is some distance off, and therefore cannot get here. A cube foot he makes the experiments upon—and wonderful experiments they were. These laboratory experiments gave birth to a system called intermittent downward filtration. Just let us see the conditions, and I will represent things as fairly as I possibly can. First, he says, you must have "suitably constituted soil." I am using his own words. It is difficult exactly to say what he means by "a suitably constituted soil," but I suppose I shall not be far wrong in saying that he means a soil not too open, so that anything may pass, and not too close, so that nothing will pass. Then he says (and let us be clear about this) as the essen-

tial part of intermittent downward filtration you must have deep draining, and that is fixed at 6 feet. I am using his own words, because his calculations are made upon 6 cubic feet, so as to allow a considerable distance for percolation. So that, if one foot will do so much work, six times that number will do so much more. This is Dr. Frankland's principle of filtration as opposed to irrigation, but it is intermittent. Dr. Frankland says there is something like a lung action—a constant taking in and giving out; he says he copies Nature. Dr. Frankland makes his experiment on this cube foot of earth, and inasmuch as this cube foot would do so much work, he calculates what six cubic feet would do, one on the top of the other, and he calculates 3,300 persons to the acre. He says I have got a new intermittent downward filtration, and I can put a large bulk of sewage on a small plot of land. Now, let me illustrate this with a case. I am going to try to give you an account of what, I suppose, is the intermittent downward filtration. Suppose I had a population of 9,900 on three acres; subdivide each acre, as he says, into four parts; put on each of those four parts your sewage of 3,300 for six hours; take it off, put it on another part, that gives 18 hours' rest to each part of your acre. That is the intermittent system. Of course there were people who went far beyond Dr. Frankland. We had a further development suggested. I fancy it was suggested that of those three acres the one should be used one year, and that the sewage of the 9,900 people should go upon that for that time, while the others rested. That might very fairly and properly be called intensified intermittent irrigation. I do not think I am using a wrong phrase. Let us not in any way misunderstand Dr. Frankland. I am not saying a word against him, because as a matter of fact no one knows the subject better than Dr. Frankland. But of this intermittent downward filtration, Dr. Frankland said that it involved the sacrifice of agricultural property. It is fair that we should be distinct about that; he always said it involved that sacrifice. I refer to that because some of his followers take a different view; my friend, Mr. Bailey Denton does; I know he thinks otherwise. He thinks that intermittent downward filtration is the best way of getting a good crop.

Now, I want to consider a new aspect of this case; the sewage without the removal of the sludge (because this is the principle of Dr. Frankland) is put upon the ground. But two insuperable difficulties occur, and I am now speaking of the results of my own experience on this subject. The first is that, as the result of this application of sewage to land, you destroy your land as a filtering agent; that it is not, at the end of a year or two, what it was when you commenced; and, secondly, that it is almost certain that you have a nuisance. I am going to take these two points, and enlarge upon them. First of all, then, I would note that Mr. Bailey Denton (and he knows this subject exceedingly well) contends that when you put that sewage with the organic matter in it upon the land, and dig it in, as he says you ought to do—it undergoes decomposition—renders the ground lighter and better by increasing its filtering capacity. That is Mr. Bailey Denton's contention. From that statement I absolutely and entirely differ. Now, just let us consider it. The sewage is applied, with the matters in suspension, to the ground. For 18 hours you apply none. The fact is that you get over a certain surface of your ground a covering which is practically impermeable, and which is not removed at once when the sewage is again applied to the land. It is curious what it is. There is first of all a considerable quantity of albuminous matter, but it is something else. We know the immense quantity of paper that goes into the sewage; now all this disappears. That is a remarkable fact; but what becomes of it—it gets beaten up into a kind of *papier maché* in the sewage; and it is that *papier maché* I think which clogs the ground; you dig it in; it simply fills up the pores with a glutinous and *papier maché* cement, and you may put any amount of sewage on you like, but it will not go through. I know that to be true, for I have tested the subject with every possible care as to the relative power of earth for allowing sewage to go through after its application. It clogs the land; your land deteriorates. Now comes the 3,300 per acre. I suppose there was no greater authority than Mr. Bailey Den-

ton on intermittent downward filtration. When he had to treat the sewage of a population of 13,500 at Kendal, he suggested 16 acres of land for the process; but the authorities did not see this, they had the report of the committee before them, and they said 3,300 to the acre. They were very liberal to him, and suggested 5½ acres, but it did not answer. I must say, I do not like to enter upon this now, but the letter of Dr. Frankland in to-day's *Times* has rather astonished me; he has come down, at any rate, to 2,000 to the acre; but I want to show what an extraordinary result we have got. Dr. Frankland's results were based on this—a cube foot of earth would purify a certain quantity of sewage, and, therefore, six cubic feet, one under the other, should purify six times the quantity. He tells us here that this conclusion is drawn from the Warrington experiment, and that, inasmuch as it proved that the purifying action only takes place near the surface of the land, it is only necessary to drain the soil something like 2 ft. deep instead of 6 ft.; but where have his own experiments gone? His own experiments were based upon the cube foot of earth; the power it possessed to purify a certain quantity of water multiplied by a certain number, but now he has abandoned it. Dr. Frankland's letter has rather put me out. I do not know where we are.

I cannot help coming to the hygienic aspect of the subject, because it has a medical aspect too. First of all, we cannot hide from ourselves the nuisance from offensive emanations on the ground; we have seen it over and over again. The evidence is abundant. Can it be otherwise? Injurious matter on to the ground, the water allowed to drain, and its material kept upon the surface; evaporation occurring; evaporation means the carrying, not simply of water in the air, but a considerable quantity of solid matter, that is carried off during the time of evaporation. For instance, you go into a room newly painted, and you get lead poisoning. How? It is not due to the volatilization of the lead, but the turpentine, in the act of evaporating, carries solid lead into the air. That is the explanation of the lead poisoning; when the smell has gone the danger has passed. What do you do here? You have a thing which of all others you want to keep out of the air, and you spread it over a large surface of ground. I will not enter into another aspect of the question, which, from a medical point of view is immensely important—I mean the saturation of a district with moisture. This would not be the place to enter upon it, but I may say the masterly research of Dr. Buchanan has clearly shown the danger of that.

Then we have pollution of subsoil water; we have the distribution of undefecated sewage, containing the ova of entozoa and other things which I might mention. That is irrigation. I want to be very careful about what I am saying. I do not for one single moment mean to say that there are not cases where irrigation should not be properly used. When I come to discuss, as I hope I shall before I have done, the question of how you are to treat your sewage, I shall emphatically say you cannot treat it this way or that, but you can treat it in various ways at different places, and irrigation may be one of them; I am only saying that irrigation is not the way to meet it universally—it cannot be.

Now let us review our facts. We have dilute sewage to deal with; we desire to be sanitarians, to purify our sewage, so that it shall not pollute our watercourses, and in purifying it not to cause a nuisance. We desire to be economists, viz., to get from the sewage all that is valuable in it—in a word, to achieve by one and the same operation a sanitary success and a commercial profit. In sewage, as in other things, you cannot combine the incompatible. To achieve your commercial success, you must abandon sanitary considerations, and you must, as at Edinburgh, flood your land with 10,000 tons per acre per annum, until your farm is a stinking morass and your effluent water so impure that you must take it directly into the sea lest it foul your watercourse. Achieve your sanitary success, sprinkle your 300 tons on your acre with hose and jet, and away goes agricultural profit.

Try a compromise between the extremes of 300 and 10,000, and the chances are you get the difficulties of both and the advantages of neither. I admit possible exceptions. Do not misunderstand me. I say again I admit

possible exceptions. A small population, cheap land removed from human habitations, a porous soil admitting free percolation, good gradient not requiring steam power, proximity to the sea, so that extreme purity of effluent is not demanded, and proximity to a town, so that a ready sale for the rank sewage grass for dairy purposes could be secured. But the difficulties are enormous. I must have enough land, and the greater the population the greater the quantity of land required, and the larger its price. I must have proper land, sufficiently porous, but not too porous. I must have land properly levelled and properly drained. If the level of my drain be above the sewer outfall, I require costly motive power. The larger the quantity of sewage, as in wet weather, the less able is the already overcrowded land to deal with it. Frost or snow, the work has still to be done. At all times the effluent must be sufficiently pure, lest litigants be aroused. At all times the operation must be conducted without offensive effluvia from an over-sodden state of soil, and without polluting the air by rendering it abnormally damp or affected by sewage effluvia. Without polluting the air, I say, for that means typhoid. The subsoil water must be so diverted that neighboring wells shall not be polluted. Grant all these difficulties overcome, and there remains in the produce of my farm a grass unfit for dairy purposes, and likely to be a source of entozoic infection to men.

Now I come to precipitation. Let us understand what precipitation is:—The addition of certain chemicals whereby the deposit of the solids in suspension, and some of the dissolved matters is effected, together with the deodorization of the offensive constituents. I have taken the trouble to go through, so far as I can make out, nearly all the patents which have been suggested for the purpose of precipitation, and dry work it has been. I have endeavored to find out where they have been used; and I have got information from the different places as to the results. I think you may say this, that those agents principally employed are practically lime, salts of magnesium, salts of alumina, salts of iron, alone, or mixed up one with the other, in all sorts of proportions; whilst in addition some people put a certain amount of weighting matter to carry the precipitate down, and others an absorbent matter to absorb the gases. I want to put it that there are, to my mind, five conditions as to a proper precipitant; first, that consistent with purity, the chemicals used should be cheap. Secondly, that the precipitants should act as deodorisers and disinfectants. Thirdly, that the maximum purity should be obtained with the minimum of deposit, that is in the smallest possible quantity of chemicals. Fourthly, that the precipitated matters should subside rapidly. Fifthly, that the sludge should part with its water rapidly. I think these are the five conditions of success so far as precipitants are concerned. I have here some tanks which have been kindly sent to me by the A B C Company. In one we have the crude sewage and in the other the effluent, and I will ask Mr. Page to add some of the A B C material to the sewage in one of these tanks, in order that we may see how the precipitating action goes on; but I wish it to be understood that I am not here to advocate this or any system. The A B C system, I will say at once, produces in my own experience, and in that of Professor Dewar, who was conjoined with me in a research on the subject, a very good effluent. I am not going to say any more than that. Those who have had an opportunity of investigating the subject will admit that it does produce a very good effluent; that is all I say.

Now, I come to the practical question, the question asked, I would say, all over the country, and the question asked of those who have to advise on these matters, in tones almost of painful despair. How shall we deal with our sewage? I want to say this, there is no one single answer to be given to that question. The adviser must, if he wishes to do justice, sink his hobby. I do not care whether it is the hobby of precipitation or the hobby of irrigation—he must sink his hobby. He must be prepared to find conditions under which he would advise irrigation, and conditions under which he would advise precipitation. And let me say he is certain to find (in my own experience they are to be found) conditions that are favorable to one hobby at one place, but are absolutely inconsistent to that hobby at another. I want to

say this, too, that it is no good people holding up success at one place as an argument that that process will be successful everywhere. It is no good to have sewage works at A, and to take people from B, C and D down to those works at A to see how successful they are. That will not do. We have to consider the place, and we must be prepared to adopt any process that is the best fitted for that place.

Let us consider one or two general points. I am going to quote the words of my friend Sir Robert Rawlinson, "towns must be prepared to pay to be clean." That is what he said once in this place. You must purify your sewage at the expense of the rates.

[Sir Robert Rawlinson took exception to the expression "purify." "Clarify" was the proper word.]

I do not argue about the word; towns at any, must pay to be clean. Secondly, and I think Sir Robert Rawlinson will agree with me in this—I know I am on tender ground with him—the sewage must be got rid of—I am sure he will let me use my word, purified—at any cost. Minimise the cost, he would say, but you must do it at a cost. and there must be an annual expenditure. Thirdly, and I think we shall agree here, that the sewage requires constant attention; you cannot be attending to it one month and leaving it the next—you must be always at it.

I would say this, it is not simply applying to the Local Government Board, and having an inspector to go down and see whether he will grant borrowing powers for certain works. That is all right enough, Local Boards do not mind doing these things sometimes; but Sir Robert Rawlinson will tell you, I am sure, that there is something else wanted. It is not the borrowing of ten, twenty or forty thousand pounds, but it is the yearly expenditure to keep up those works that is necessary. Fourthly, to allow your sewage to go into a river means passing your filth on to your neighbor. The sewage ought to be treated, in my opinion, where it is produced. Litigation is, in my experience —although I may have derived advantages from it—a more expensive luxury than sewage treatment, and on the whole I think it breeds far more ill-will. Fifthly, I think Sir Robert Rawlinson will go with me here—that in treating our sewage we do not want a drinking water, and we do not want a Peruvian guano. Now, we go to a town, and the sewage has to be treated. We ask how much there is to treat, and what sort of stuff is it?—that is to say, what are the manufactures, and so forth. Our very first thought necessarily is, and it would be my thought first—irrigation. The local authorities must get land. It is no use talking about farmers; you must get land. To get land is a very difficult thing; and when you get land, you have to pay a fancy price for it generally, especially if it is for sewage. And there is something else about it; that all the people who have land in the neighborhood of your proposed sewage farm suddenly discover that all their land is building land. Now come certain questions. Is it practicable? And I think I may put it thus. There are three questions we ask ourselves, supposing we think about irrigation. Is it practicable to obtain sufficient and properly situated land for the purpose; sufficient—I am not going to define that now; properly situated as regards general character, level and so forth. Secondly, is this properly situated land reasonably near to the town, so that the cost of conducting the sewage shall not be excessive; reasonably distant, so that the town may not derive injury from a nuisance likely to cause disorders. And then, thirdly, is the land of such level that the sewage can readily reach it? There is a fourth question, Where is the effluent to be discharged?—and I beg to submit that this is a very important question in any irrigation scheme. You are dependent for effective purification on effective land in effective order. But land becomes ineffective from circumstances over which you have no control. First, for instance, frost; second, in times of heavy rain, land is water-logged, and when there is most sewage to be treated, your land is least able to treat it. If, then, the outfall be into a river where considerable purity is required, and not only purity, but unfailing continuity of purity, then it appears to me that an irrigation scheme must be regarded as an unsafe scheme. But where there is a discharge of an effluent, or the discharge of a little

doubtful water at times, or a little doubtful sewage, that is a matter of secondary importance, and it appears to me your irrigation scheme may pass.

My own experience of all kinds of schemes (and I think I have seen most) has led me to prefer by a very long way indeed a combination of a precipitation and an irrigation scheme; and I see for it two great advantages. First, its efficient working is independent of weather; second, if you have sufficiently large works, any emergency of quantity can be met. I am bound to say this, looking over the papers of people who have written on this subject of precipitation—and precipitation has its greatest and heaviest enemies in its advocates—one is perfectly astounded at the extravagant advantages which have been claimed for precipitation. The sludge has been advertised, and is advertised, as of enormous manurial value. Patents by the hundred have been taken out. Precipitation advocates have invariably been the advocates of a system, and no doubt the public began to doubt everybody and everything. They have taken the claims advanced by the precipitationists, and they have weighed them in the balance of facts, and they have found them wanting. I think you will believe, after that statement, that I am not here as the advocate of a system. I said the A B C turned out a good effluent, and so it does. My own experience leads me to speak very highly indeed of the use of lime, and sulphate of alumina. Put in your lime first. Your lime does three things—it produces carbonate of lime, acts as a weighty material; some of the lime is combined with some of the organic matter in solution, something like a quarter or a fifth of it, into an indescribable chemical compound. The rest goes to make the water alkaline. Then add the sulphate of alumina; the alumina sets free the ammonia, which combines with the sulphur; it acts as a mordant, and down it goes. That is not a patent. Now, I am bound to say this—I have never yet seen an effluent from a precipitation process that has not a slight smell about it; but it is not the smell of sewage, it is the smell of effluent, *sui generis*. Now, let that effluent, after precipitation, flow over a small area of land, and you give your effluent the final finishing touches towards purification. Just think of it from three points of view. You cannot cause a nuisance in that act because you get no suspended matter, and you get nothing on the surface that will decompose and give off offensive matters. You cannot clog your ground, because the materials that were in solution are precipitated. Thirdly—I attach no importance to this, because I do not think anything of it; there is a certain manurial value. The quantity that it takes varies with the land—about 5,000 to 10,000 per acre will do. Such sewage works are no nuisance. Your process is carried out in wells, your tanks are little more than ordinary pure water tanks, with no more smell; and your sewage, when it gets on the land, has no smell either.

This subject of precipitation has occupied me a very great deal, and I venture to put forward five points to which I wish to draw your attention. First, if you want to treat sewage properly by a precipitation process, you must treat it fresh. I am not going to define what "fresh" means, but I mean before active putrefaction sets in; it may be twenty-four hours or it may be forty-eight hours; it varies with the time of year, or with the chemicals that are allowed to go in from various manufactories. Of course one change has probably occurred, and that is the breaking up of the urea. I do not know anything more wonderful than the absolute disappearance of urea; by the time the sewage reaches the works an enormous quantity must be broken up. Then there are all these different decompositions, and you must deal with it before the first active change occurs. As a matter of fact, fresh sewage has very little smell at all. Then, secondly, I think it advisable, as a rule, before you mix your chemicals with it, in order to minimise your chemicals, that you should strain your sewage in some way or other. You cannot strain it entirely, but Mr. Baldwin Latham's apparatus seems to work exceedingly well. I have seen it working, and some method of that kind should be adopted. Then, thirdly, you should add sufficient chemicals to effectually complete purification. It is a great mistake to starve your chemicals. I know as a fact that local authorities will spend money on work s

but when it comes to actual working year by year, and day by day, then it is they fall through. My friend, Sir Robert Rawlinson, knows that is the test; it is not the borrowing of £40,000, it is spending £800 or £900 a year in doing the work after they have got the works.

I do not like to enter on this subject of sufficient chemicals, especially in view of a report I have here. I have very strong views about it; but will not enter upon them now. I will only say this, that I am astonished that, when I proposed in a certain scheme five grains of lime and five grains of aluminum, one of the gentlemen who signed this report showed to a certain inspector how absolutely useless it was to add that quantity of chemicals to sewage, and said it would lead to no result at all. I say nothing about that for the present, though I may have something to say on another occasion and in another place. I want simply to say, with respect to these chemicals, that my strong conviction is that the quantity ought to be calculated on the population and not on the sewage. I have always felt that most strongly. Then fourthly, it is very important that there should be efficient stirring after the addition of the chemicals. You may add your chemicals, and it will produce no effect; give it a good stirring up; we chemists know the value of a stirring rod. Then, fifth, it is essential that you should have sufficient tank accommodation. I have worked that out as far as I possibly can—sufficient tank accommodation—for two reasons: 1st, that the precipitate should subside perfectly, that is important for the flocculent matter to go down; and secondly, that the sludge may be frequently removed. It is important that the sludge should be frequently removed from the tanks for two reasons. First, if you allow the old sludge to remain in the tanks, it is perfectly certain that it will contaminate your fresh sewage after it is treated, when it comes in; and you must have it sufficiently large, or the sludge at the bottom will undergo decomposition, and render the sewage impure after you have treated it; and, secondly, that when removed you may not get any smell—that is to say, that it may be done sufficiently frequently. Another thing which is very important is this, that when the sludge is taken out of the tank, the tank itself must be washed. It is those bits that get left in which cause so much nuisance. There is nothing like cleanliness in the treatment of sewage.

In these circumstances, the question arises, Have you produced such an effluent as will not pollute your watercourses or prove dangerous if discharged into a river used for drinking. I say that by combining precipitation, which will produce a good effluent with land treatment, or prepared filters, or something of that kind, you may produce the best effluent that is known. The late Royal Commission — Lord Bramwell's Commission—in a very able report, dealt with that, and stated that as their conclusion. I am not saying whether I agree with the report so far as London sewage is concerned, but as to that being the best method of dealing with sewage as an abstract question, I do agree with it.

Then, secondly, comes the question, is not the sludge an inevitable cause of nuisance? It was formerly. I am not certain that the materials in suspension were not a greater nuisance when taken out of the sewage than when left in it, I sometimes used to think they were. I will not discuss now the quantity of sludge, but I would say as a general principle, consistently with efficiency, produce as little sludge as possible; first, because if it has any value, you secure its maximum value, and, secondly, if it has no value you have the less to get rid of.

Now, I will say a few words on the disposal of sludge; and it is almost my last point. A great change has been wrought lately by the introduction of Mr. Johnson's press. It has almost revolutionized the question of precipitation; to my mind it has quite done so. This sludge was always our bugbear—we never knew what to do with it. The principle of Mr. Johnson's press is this: You have a series of compartments in which there are canvas bags; the sewage is compressed into these compartments by means of air pressure, about 120 lbs. to the square inch, and then the material is thoroughly pressed in these presses, the liquid is pressed out, and the solid part remains in the compartments as a solid cake, of which I have a large speci-

men here. It is perfectly compact, very easy to handle, and has not very much smell. I do not say it has none, but not very much. The water is reduced from 90 per cent. to about 50 per cent., and I should like to draw attention to the fact that the liquid which is expressed from this sludge—and they generally put in a certain amount of lime, which renders it more easy of pressure—is of a very foul nature. This was a matter investigated by Professor Dewar; we worked at it some time in common, and we found it was very unadvisable ever to return the liquid from the sludge press into the ordinary sewer, which was formerly the plan. It cannot be re-treated in the ordinary way; it requires a different kind of treatment, by chloride of lime, and some other things which I will not refer to now. It must not go back into the sewer, but must be treated by itself. I am quite clear on that point, and it can easily be done. It renders the sewage into which it is returned somewhat difficult to treat, besides being itself not treated. I have considered in some detail the value of this sludge, and the cost of dressing it, and I may say the dressing costs from 2s. to 2s. 6d. per ton. That, I believe, is a wonderfully practical method of doing it. I have walked about in the middle of thousands of tons of this stuff, and really smelt it very little. My own opinion is that Johnson has done a very great deal to change the precipitation works from being a great nuisance into being comparatively no nuisance at all.

It would not be fair in this room to pass over General Scott's process, which was very ingenious. His process was to take the sludge, which was precipitated with lime in this case, drain it, and then dry it by heat, and burn it in kilns, grind the clinkers, and use it as a hydraulic cement. And I believe General Scott said that the cement thus prepared was worth something like 35s. a ton. I cannot help thinking that may be all very true up to a certain point, but at the same time it is very evident that the sewage varies in different towns enormously, and that it would be difficult to get a cement of a constant character; and it is a very important thing, so far as I understand engineering work, that cement should be of a very constant character.

Then, thirdly, there is the destructor, and, to my mind, the destructor has reached its highest state of perfection at Ealing, from the great thought that Mr. Jones, the surveyor of Ealing, has given to it. His sludge there is mixed with house refuse and burnt. Mr. Jones's view is that every town produces sufficient house refuse to burn the sludge. One has to notice the differences of destructors. I have seen a good many myself, and I should say the differences are mainly two; first, a certain escape of offensive vapors from the shaft, and I think those offensive vapors are mainly due to partial burning—the destructive distillation, as a matter of fact, of the materials, instead of their complete destruction; secondly, the escape of fine sand and such like from the shaft at certain stages of the operation. I have seen those two nuisances very well marked, and I had occasion to advise on them on more than one occasion. I cannot help thinking myself that in Jones's destructor, where he places a muffle furnace or "fume destroyer," as he calls it, between the furnace and the main shaft, he has, in a great measure, met those two difficulties. I think, myself, that Jones's destructor is a very creditable thing.

I feel rather a difficulty in saying a word on the subject of standards, and yet I do not know that one can altogether omit it. I can only say that Professor Dewar and myself found out the absurdity of comparing a sample of raw sewage with a sample of effluent. People go to a sewage works, and take a sample of sewage and a sample of effluent and compare them, which is of course manifestly nonsense. You can only compare them by taking, say, half-hourly samples of effluent and half-hourly samples of sewage, mixing them all together and comparing the average. You cannot compare a sample of sewage with a sample of effluent, for this reason. Say they are both taken at twelve o'clock. The twelve o'clock sewage, say, is strong sewage, but the twelve o'clock effluent probably is the effluent of the six o'clock sewage, when it is at the weakest; so that you are comparing the effluent of a very weak sewage with very strong sewage,

or it may be the opposite. You cannot compare one sample with one sample taken at the same time, or compare them in any way except by taking averages. Of course, it is very important in taking samples by which you are to come to any conclusion that you should know the weather, the average flow, and so forth. Now one word on what may be called a practical standard. We always hear about standards, and they are not very satisfactory. We had the Rivers Purification Bill, which I am very glad to say the House of Commons in its wisdom turned out. That was a very great point, because it was a very bad bill, altogether bad both in principle and in theory. We want a bill, however, to keep sewage out of rivers, and we ought to have it. We must leave the manufacturers alone for the present. It is quite hard enough in our days for people to get a living, and we must not be interfering too much with trade. I do not mean to say that will not come, I think it will; but we cannot do everything at once. But I will tell you what we can do. We know sufficient about sewage now; the Local Government Board has taught us a good deal, doubtless. At any rate, we do know enough about it to know that we can purify it, or clarify it, up to a certain point. And I think we ought to keep our sewage out of our rivers, and it is the duty of the local authority to do it. But we cannot be having elaborate chemical tests. Let us have some common sense standard. Something of this sort I may throw out as a suggestion. First, that your effluent shall be clear and colorless when seen in a white pint cylindrical bottle. Secondly, that it should not be alkaline to test paper—you can get that, and an alkaline effluent is not a good effluent. Thirdly, that on the addition of a grain of sulphate of alumina, or say, one grain of alum, dissolved in 100 grains of water, added to the pint bottle, it shall not produce appreciable turbidity after standing thirty minutes—you can get that. Fourthly, that if you take that white cylindrical bottle half filled with sewage, and shake it up, it shall not leave foam or much froth after standing ten minutes. These are practical tests.

I do hope the day is not so far off when we shall have some means by which we can persuade local authorities to purify and deal with their sewage. But I say again that if we are to do it, we must not act as the creators of a hobby, we must be prepared to sink the hobby; we must not go as violent irrigationists, because it is clear that that will not do; we must not go as violent precipitationists, it is clear that will not do. We must be prepared to consider what is the best method to treat the sewage of the place for which we are called upon to advise.

I cannot help feeling strongly on one other subject. I know we cannot alter it; we have this water-carried sewage to deal with; but one cannot help asking the question, if one were called upon to advise for another London, or in another planet, should we advise the water-carriage system? I have thought this subject over very earnestly lately. The advocates of the water-closet system urge that water, as a vehicle to carry the refuse, commends itself to us on the ground of cleanliness and cheapness. They would compare, and do compare, the natural power of gravitation, such as is made use of by the water-closets, with an organization of men and carts such as is required, for instance, by the midden system. I confess the advantages at first sight are all on one side, but I must say there are some facts which point in the opposite direction. Diluting with water is the very best known method of rendering whatever is valuable in sewage practically worthless, and sometimes an ungovernable nuisance. The excreta of animals are no doubt intended for the food of plants, and, again, for us through their intervention. Of course, do what we like, do what we please, nature will assert herself and assert her plans, although it is certain we do our very best to embarass nature by meddlesomeness, but the nutritive food of the plant we drown with water, and then our ingenuity fails to deal with the filthy mixture. We cannot utilise it unless we abandon all sanitary precautions. It pollutes our air; it may render our ground a simple stinking morass, and defile our watercourses. Let me put it to you; here in London 30 gallons of water per head is brought from pure sources, let me say, at great cost, with vast engineering skill, filtered, and per-

haps refiltered with extraordinary care, stored with scrupulous anxiety, analysed by one chemist after another, and what for? About one-ninetieth part of the water is used for drinking purposes, and a large quantity is destined to be the mere diluent of our sewage, to perplex us by its uselessness, and to steal our health with the perpetual nuisance it creates.

EXAMINATION AND TESTING OF BERLIN WATER SUPPLY.

From Abstracts of the Institution of Civil Engineers.

The testings of the Berlin water during the period from July 1, 1884, to April 1, 1885, have been regularly carried out, in accordance with the plan agreed upon between the Director of the Municipal Waterworks, Mr. Gill, M. Inst. C. E., and the civic authorities. The chemical analysis was arranged as follows:

1. Estimation of the residue from the evaporation of 200 cubic centimetres of water, dried for five hours at 110° Centigrade.

2. Estimation of the volatile portion of the residue by calcination, moistening the ash with carbonate of ammonia and again calcining it at a low temperature.

3. Estimation of the chlorides by titration of 200 cubic centimetres with $\frac{1}{10}$ normal silver solution, according to Mohr's system.

4. Estimation of the ammonia (after separation of the lime, magnesia, iron, &c., by means of soda-lye and carbonate of soda) by calorimetric tests of the liquid decanted from the residue, with Nessler's solution.

5. Estimation of the lime by titration of the amount of oxalic acid necessary for the precipitation of the lime, by means of chameleon solution, on Mohr's system.

6. Estimation of the oxidizability in a solution of sulphuric acid, on boiling for ten minutes, on Kubel's system.

The investigations for nitro-organisms were both microscopical, and by means of pure cultivation on a solid medium (10 per cent. meat juice-pepton-gelatine).

Few variations in the chemical composition of the water of the Spree and that of Lake Tegel were observed, but the contents in micro-organisims fluctuated considerably. The Spree water is richer in chlorides and in substances capable of being reduced in an acid solution by the chameleon tests, also in fertile germs of micro-organisms. An appreciable amount of ammonia could only be found in the Spree water, and this reached on one occasion 0.23 milligram per litre. The water of Lake Tegel only showed on one day a measurable quantity of ammonia, viz.: 0.04 milligram per litre. The efficacy of the sand filters is next examined. On all occasions the contents in organic matter, chlorides, the oxidisability and micro-organisms were reduced by filtration, but the dissolved lime was slightly increased. The tests on March 2d showed that the filtration had only reduced the fertile germs in each cubic centimetre from 963 to 468, whereas in the previous week the reduction had been from 210 to 68, and in the previous fortnight from 250 to 28. This led to an exchange of correspondence with the Engineer, and to the discovery that, owing to the formation of a thick sheet of ice over the uncovered filter-basin, it had been impossible to cleanse it.

Daily examinations were made of the water from each of the filter-beds, to test their action with respect to yield, endurance, rapidity of yield under pressure, and influence of temperature, and these tests were made in comparison with the unfiltered water. Comparative investigations were also made respecting the efficacy of open and covered filter-beds, and the fact was established that the open filter-bed removes considerably more fertile germs than the covered one. Precautions were taken to test most carefully for the *crenothrix polyspora*, both in the freshly-collected samples and in those which had stood for twenty-four hours, but on no occasion was the presence of this conferva detected by microscopic examination of the water from the

Spree. It was, however, often present in the water of Lake Tegel. The investigations generally prove that the water-supply is rendered more or less impure after leaving the source, either by the distribution through the pipes or by storage in the high-pressure reservoir at Charlottenberg. Nothing, has, however, transpired to indicate, either chemically or microscopically, the presence of injurious agents of any description in the water of Berlin.

LOGARITHMIC AND RIBBED OBLIQUE ARCHES.*

By JOHN L. CULLEY.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

CHAPTER VII.

LOGARITHMIC METHOD.

69. This method of constructing oblique arches is so-called because naperian logarithms are used in their calculations.

The soffit coursing joints by this method are always normal to the plane of the arch face, wherever they come in contact with it, and hence it is these coursing joints are normal to any plane parallel with the arch faces at their points of contact in the parallel plane. The soffit heading joints are elliptical curves in planes parallel with the arch faces, and are, therefore, normal to the coursing joints of the soffit at their intersections. The heading and coursing joints of the soffit being thus normal to one another, they will also be normal to one another in the developed soffit.

70. Fig. 36 shows the plan and development of the soffit of a semi-circular oblique arch, whose elevation and right section is shown at A B C and H I J. The curve N X′K normal to the curve S X′G at its middle joint X′ is the developed soffit coursing joint through that point. Through the middle point R of the spring line S O, draw the dotted line R P parallel to the curved ends of the soffit S G and O Q. Divide R P thus drawn in to any convenient number of parts of equal length (Fig. 37) and through the point of division, draw their coursing joints parallel to the curve N X′R. The widths of the courses are thus determined on the middle curve R P, in order to show the same order of arrangement and of size of the several courses in the arch faces, but their dimensions may be fixed on any other parallel curve to R P. It will, however, be found most convenient to take the middle curve R P, and also to make the courses of one width on this curve.

71. Having thus determined the position of the coursing joints in development, the heading joints are drawn in the several courses at desired or convenient points, and their elliptical

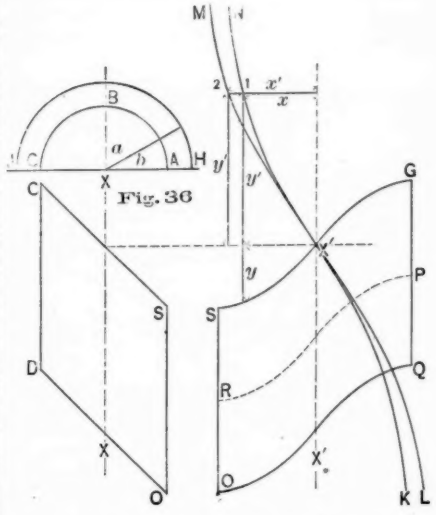

Fig. 36

curves are drawn parallel to R P, or to the curves of the developed arch ends. It should be borne in mind that the heading and coursing joints in the development are drawn parallel with R P and with N X′R on lines parallel to the spring lines S O and G Q. Thus, if we cut out a cardboard templet, one of whose edges

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will correspond with the curve N X′K and its left-hand edge is straight and parallel to S O, and through the points of division in R P we draw the several coursing joints shown in Fig. 37, this templet should be moved so that its left hand straight edge shall always be parallel to the spring line S O. In like manner the heading joints may be drawn with

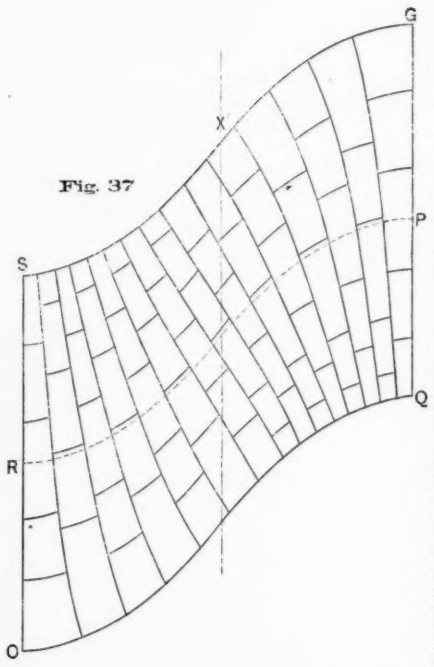


Fig. 37

a templet of curvature S X′G, but in moving it over the development S O Q G, its extremities S and G should always move on the spring lines S O and G Q, whilst the curved face of the templet moves parallel to the end curves S G and O Q. These precautions should always be observed, otherwise the curved lines drawn would not be correct or in accordance with the requirement of article 69.

72 The equation of the normal curve N X′K to S X′G at its middle point X′ will now be determined.

In article 41 will be found the expression

$$y = r \sin.\ a \text{ tang. } \theta \qquad (32)$$

Now since x is dependent upon a for its value this equation contains a ratio of x to y, and is therefore an equation of the end curve S X′G, and might be used for it in place of equation (17). Let b be the complement of a, and we have

$$y = r \cos.\ b \text{ tang. } \theta \qquad (33)$$

Again a is usually expressed as so many degrees, or as W. 180°. It is in fact the length of an arc of n 180° to radius unity. Thus, the expression tang. 36° means the tangent to an arc of 36° to radius unity, but not the tangent of 36°. a therefore equals $n\,\pi$,

$$\text{but } x = n\,\pi\,r, \qquad \therefore\ x = r\,a \qquad (34)$$

$$\text{but } a = \frac{\pi}{2} - b, \qquad \therefore\ x = \frac{r\pi}{2} - rb \qquad (35)$$

Differentiating equations (33) and (35) and dividing we have:

$$\frac{dy}{dx} = -\frac{r \text{ tan. } \theta \sin.\ b\,db}{r\,db} = -\text{tan. } \theta \sin.\ b \qquad (36)$$

Now, let y' (Fig. 36) be the ordinate to any point in N X′K that y is for the corresponding point in S X′G for the ordinate x. At an infinitesimal distance from the point of contact X′ the curves N X′ K and S X′G are straight lines. Thus, in Fig. 38, let X′ be the origin of co-ordinates X′Y the axis of y, and X′X that of x. Now, if on X X′ we lay off an infinitesimal distance X′B, and through B draw A C parallel to X′Y, the curves A X′ and C X′ are exactly at right angles to one another within these limits, and the ordinates to A will be:

$-dx$ and dy' and the ordinates to C will be

$-dy$ and $-dx$. The triangles AX′B and BX′C are similar, whence

$$-dy : -dx :: -dx : dy',$$

$$\text{or} \qquad \frac{dy}{dx} = -\frac{dx}{dy'} \qquad (37)$$

$$\text{but } \frac{dy}{dx} = -\text{tang. } \theta \sin.\ b \text{ (see eq. 36)}$$

$$\therefore \frac{dx}{dy'} = \text{tan. } \theta \sin.\ b \text{ (38). But } dx = -r db$$

$$\text{or } dy' = -\frac{rdb}{\text{tan. } \theta \sin.\ b} = -\frac{r}{\text{tan. } \theta} \cdot \frac{db}{\sin.\ b}$$

$$= -\frac{r}{\text{tan. } \theta} \frac{db}{2 \sin.\ \frac{1}{2} b \cos.\ \frac{1}{2} b}$$

$$= -\frac{r}{\text{tan. }\theta} \cdot \frac{db \cos.^2 \frac{1}{2} b}{\text{cot. } \frac{1}{2} b}$$

$$= \frac{r}{\text{tan. }\theta} d \text{ log. cot. } \frac{1}{2} b \qquad (39)$$

$$\therefore y' = r \text{ cot. } \theta \text{ log. cot. } \frac{1}{2} b + C \qquad (40)$$

or, substituting $(90° - a)$ for b we have:

$$y' = r \text{ cot. } \theta \text{ log. cot. } \frac{1}{2} (90° - a) + C \qquad (41)$$

as the equation of the curve N X′K in which if

$a=0$, $x=0$, and log. cot. $\frac{1}{2}(90°-a)=0$

or $C=0$,

whence the equation of NX′K becomes

$$y' = r \text{ cot. } \theta \text{ log. cot. } \frac{1}{2} (90 - a) \qquad (42)$$

wherein, if a and $x=0$, $y=0$

if $a=90°$, $x=\frac{\pi r}{2}$ and $y=-\infty$

if $a=-90°$, $x=-\frac{\pi r}{2}$ and $y=\infty$

By the aid of Eqs. (42) and (32) the soffit coursing and heading joints in the development may be determined with great precision.

73. *The coursing beds of oblique arches by the logarithmic method are generated by a radial line normal to, and moving along the axis of the arch as one directrix, and on a cylindrical curve as the other directrix, which at all points is normal to planes parallel to the arch faces.*

This second directrix is usually taken in the soffit, and we will so treat it here, but should be in cylindrical surface, midway between the intrados and the extrados.

74. The great similarity in the generation of the coursing bed surfaces by the helicoidal and by the logarithmic methods. It should be noted both are generated by a radial line normal to, and moving along the axis of the arch. Their difference is in the fact that, in one case the second directrix is a helix, and in the other, a normal curve to the arch faces. It matters little what this second directrix is so long as its curvature is known. But it is of the greatest importance that we keep in mind that the first directrix is radial in the logarithmic just the same as it is in the helicoidal method; nor should this idea, in the treatment of oblique arches by either of these methods, be ever lost sight of. It is the fundamental principle and renders these two methods quite similar in construction, and for this reason the treatment of logarithmic arches is readily understood when the problems connected with helicoidal arches have been once mastered.

75. Again, it should be noted the only straight line elements in the coursing beds by either methods are radial; that is, they are in lines normal to the axis of the arch, and consequently, are always normal to the coursing joints, both intradosal and extradosal. The only straight lines in the soffit, by either methods, are lines parallel with either the axis or the spring lines of the arch, nor should this fact be lost sight of.

76. Now, if NX′K (Fig. 36) is the intradosal joint and MX′L the extradosal joint of a coursing bed passing through X′ in the development, and if through any point 1 in NX′K we draw 1 2 in direction perpendicular to $s_1 o$ continued, the point 2 in MX′L is the intersection of the radial element through 1 in NX′K. We have already determined the ordinate of 1 to be

$$y' = r \text{ cot. } \theta \text{ log. cot. } \frac{1}{2} (90 - a) \qquad (42)$$

but y' is also ordinate of the point 2, and Eq. (42) is therefore the equation of MX′L when

$$x' = r'a \qquad (43)$$

Chapter VIII.

Method of working the voussoir.

77. Reduce the face of the stone to be worked to a true cylindrical surface by aid of the soffit templet, Fig. 25, in the manner as described in article 57. This reduction may be accomplished in a variety of ways, but the method there described is believed to be the simplest and best.

Let A B C D, Fig. 39, be the soffit thus reduced, and let A′B′C′D′ be the developed soffit of the voussoir to be worked. Through B with the straight blade of the soffit templet draw the element B E, and through B′ in the development draw corresponding line B′E′ parallel to the spring lines, or to the axis of the arch, and through D′ draw D′F′ parallel to B′E′, and through A′ and C′ draw A′G′ and C′F′ perpendicular to B′E′ and to D′F′, respectively. Lay off on B E, B G and G E equal to B′G′ and to G′E′.

Then with the curved blade of the soffit templet through G and E draw the circular arcs G A and E C, whose respective lengths shall be equal to A′G′ and E′C′ in the development.

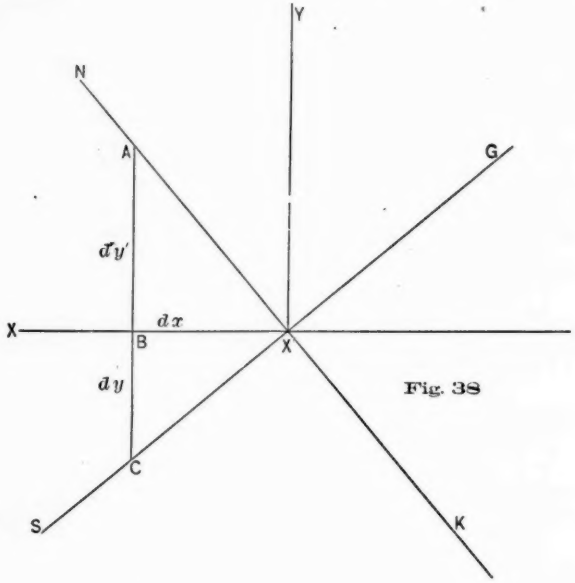


Fig. 38

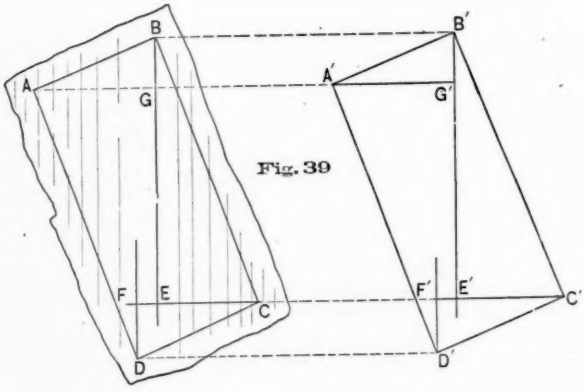


Fig. 39

Produce the arc E C to F and make E F equal to E′F′ in the development, and with the straight blade of the soffit templet draw the element F D parallel to B E, and make F D equal to F′D′ in the development. The four corners A, B, C and D of the voussoir soffit are now determined.

78. The heading and coursing joints of this soffit are reduced in a simple manner. Thus let a flexible rule of cardboard, thin hard wood, or other suitable material, be cut, with one edge of the exact length and curvature of the developed joint A′B′, and apply its extreme points A′ and B′ at A and B, press the rule against the cylindrical soffit, and

cause it to thoroughly conform to this surface, and whilst so applied, work on the soffit between A and B the line of the curved edge of the rule, and the joint A B will be determined. The joints B C, C D and D A may be thus determined by rules cut to their developed curves, and then applied to the worked soffit of the voussoir in like manner as A B was determined.

79. Another way of determining the voussoir corners and joints in the worked cylindrical soffit is: Let any corner B, Fig. 39, be selected as before, and through it draw the element B E. Then cut a templet of cardboard, or of other flexible material, to the exact size of the developed soffit A′B′C′D′, and draw across it the line B′E′ parallel to the spring lines of the arch, and apply this templet with its corner B′ at B and its line B′E′ on B E, and cause the templet to conform throughout to the worked cylindrical soffit surfaces, and when so applied, work all the edges of the templet on the stone, and thus determine all the joints of the soffit at one operation.

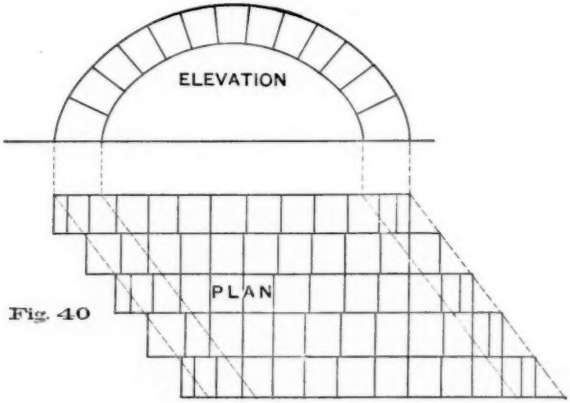


Fig. 40

REDUCTION OF THE COURSING BEDS.

80. When it is remembered that the coursing beds by this method (logarithmic) are generated by a radial line, it will at once appear that the coursing bed templet, Fig. 28, is as applicable to logarithmic arches as to helicoidal arches for the reduction of the coursing beds, when the coursing joints have been located upon the worked soffit of a voussoir.

81. The surfaces below the joints A D and B C, Fig. 39, are therefore worked to the radial edge of this templet. When so worked, radial lines are drawn with this radial edge on the coursing beds through the corners A, B, C and D, thus determining the lines of intersections of the heading and of the coursing beds of the voussoir. The heading surfaces are then worked to these radial lines so drawn, causing the surfaces to be normal to the soffit, and therefore normal to the coursing beds.

82. Much has been said as to what should be the character of the heading coursing surfaces, both by the helicoidal and by this method. It has been maintained that these surfaces, by the logarithmic method, should be planes parallel to the arch faces; that the strength and stability of the arch demanded it, &c. It in fact matters little, whether these surfaces are parallel to the arch faces or are warped surfaces, normal to the coursing beds. It is, however, the author's opinion that the latter construction is the more stable. It has the advantage, also, of simpler construction.

83. The arch face stones are to be worked precisely in the same manner as described in Articles 63 and 64 for helicoidal arches. In the one the coursing joints are helicoidal, whilst in the other these joints are curves, normal to the arch faces. The same principles are in-

volved here as there, and therefore do not require a second demonstration.

84. The logarithmic method of oblique arch construction is one that requires great care and constant supervision to successfully execute. It should be done in the most systematic manner. Thus it will be noticed that the courses increase one side of the arch while they decrease in thickness on the other side of the arch from one end of the arch to the other. Yet, two courses beginning at opposite ends of the arch at the same height above the spring line are exactly alike in all their dimensions. Their voussoirs may therefore be worked in duplicate, which, in fact, is the proper way to treat this class of construction. These two courses may be laid out at once, and all their voussoirs worked. There is no reason why all the voussoirs of the entire arch may not be worked out before any of them are set in position in the arch. All the voussoirs of a course should be kept separate and distinct by themselves. Every voussoir should be marked in plain figures or characteristics, indicating at once its course and position in it.

85. The coursing joints of every course should be marked permanently on the centering, as a guide for the voussoirs as they are set in position. This is best done by transferring to the centering ordinates taken from the development plan of the arch soffit.

RIBBED OBLIQUE ARCHES.

86. Oblique arches are sometimes constructed by placing several narrow elliptical arches or ribs, as they are called, together, as shown in Fig. 40. This method is very faulty, and cannot be too severely condemned. There is no bond between the several ribs, as each rib is separate and distinct in its construction and its position; the load above the arch is never uniform throughout the whole length of the arch, and on account of this lack of bond in the arch, it will be distorted by its unequal settlement. Again, the outer ribs are constantly being forced outwards by the action of frost upon the material that finds lodgement between their heading surfaces. So serious becomes this weakness that the ribs have to be reconstructed to restore the stability of the arch. True, these ribs are sometimes bonded one to another with iron straps, yet this bungling device is devoid of transverse strength and is susceptible of taking up the longitudinal stress only. Such an arch is not to be compared to an arch of bonded courses. Again, if it is properly bonded with iron straps, a ribbed elliptical arch costs more than an oblique arch constructed in accordance with the rules of any one of the recognized methods of proper construction.

THE FACTS AND SCIENCE OF WATER SUPPLY.

From "The Engineer."

A MASS of interesting particulars with respect to the water supply has been presented by Colonel Sir Francis Bolton and Dr. Percy Frankland, in the form of two lectures recently given before the School of Military Engineering, at the Royal Engineers' Institute, Chatham. These lectures refer to the collection, storage, purification, and examination of water. In dwelling on the various sources from which a supply of water may be obtained, it is mentioned that in England the three principal water-bearing strata are the Chalk, the Lower Greensand and the New Red Sandstone. Of these three the chalk has the largest outcrop, but its geographical position is such that it receives a much smaller rainfall than the new red sandstone, so that the actual quantity of water absorbed by the latter stratum is probably the greater. The quality of the supply also has the merit of not being so hard. Still, we are told the chalk will always be the best source of supply, as the stratum is easy to work, and should a well fall on a hard spot the difficulty can be overcome by driving galleries from the bottom, and thus increasing the volume collected in proportion to the ramification of the

workings. The extent to which water can thus be obtained is said to be "almost unlimited." We must observe, however, that there is considerable significance in the "almost." There are ancient reservoirs in the chalk, which may yield liberally for a time, but which may ultimately become exhausted. The regular annual yield will then have to be relied upon, and this is limited, though large. Allusion is made to the intention of the East London Waterworks Company to seek for an extensive supply on the burrowing principle. This company has applied to Parliament for power to raise new capital, in order to drive several miles of tunnels underneath its own property in the Lea Valley, by which means it is hoped to be able to dispense altogether with the intake from the river Lea as a source of supply. Unfortunately there appears some risk that the company will withdraw its present bill, owing to a dispute with the Select Committee as to the appropriation of the dividends in connection with the new capital. It is to be hoped that this difficulty may be bridged over, so as to permit of so desirable an experiment in water supply as that which the company contemplates. Next week will most likely see the question settled one way or the other, so far as this session is concerned. South of the Thames we find one of the companies sinking a deep well at Streatham, hoping possibly to reach the lower greensand, failing which, the supply will be taken from the intervening chalk. The sands above the chalk also yield largely, giving up two million gallons per day at Streatham, with the probability that more may be obtained by employing pumps of greater power. The Streatham well, as many of our readers probably know, has been credited with a disastrous effect on the lesser wells in the vicinity, and, indeed, at some distance. The intercommunication between wells in the chalk is notorious, and gives rise occasionally to rather awkward consequences. Happily the law is now interpreted as protecting underground waters from pollution, otherwise deep wells in the chalk might be looked upon as fraught with more danger than open rivers. With regard to a river supply, the Thames affords a striking example of the extent to which springs in the chalk supplement the volume obtained from surface drainage. It is calculated that one-third the total rainfall of the Thames basin flows down the river, though the surface drainage of such a level district cannot amount to more than one-fifth.

The effect of filtration on the quality of water is discussed in these lectures with some minuteness. It is evident that on this point we are gaining fresh light, and fortunately our increased knowledge is directly calculated to augment our confidence in the filtering process, where that remedial method is intelligently employed. It is generally supposed that two actions take place during the passage of water through a filter—the first a mechanical one, whereby the solid particles are arrested; and the second a chemical one, consisting in the oxidation of those bodies contained in the water which are capable of oxidation, and caused by the layer of air or condensed oxygen which surrounds each particle of sand composing the filter. The lectures tell us—and most likely the utterance is that of Dr. P. Frankland—that it is impossible to say how far the action of a filter is dependent on each of these two properties. "But," says the speaker, "we rather incline to the belief that the first is the principal one, and that the chemical action, if it exists at all, is very slight." There is said to be very little evidence in favor of the chemical action of a filter, while some things point directly to its absence. On one point there is no uncertainty, and that is the necessity of cleaning filters from time to time. The only rule in this particular where the London Water Companies are concerned, is that a filter should be cleaned as soon as it requires more than a given head of water to get the proper quantity through. There is hope that a more satisfactory rule will shortly be available, and the lecturers are making investigations for this purpose. But besides the artificial process of filtration, two of the Thames companies are now using an auxiliary process of filtration through the natural gravel beds which form the banks of the Thames at Hampton. These gravel beds are at all times full of water, forming a species of underground river. Perforated pipes were laid in the gravel and connected with a pumping well in order to utilize

this water, which is always clear and bright on account of the natural filtration which it has undergone. But the supply from this source was found to be small, and in order to effect an increase it was determined to flood the bed with water direct from the Thames. For this purpose a second series of perforated pipes was laid parallel, to act entirely independent from the first, and at about thirty yards distant. This second series of pipes was placed in direct communication with the Thames, but provided with a small vertical sand filter to arrest the coarser impurities. The result of this arrangement is so far important. The yield of the pumping well has been considerably increased; 3,000,000 gallons per day of perfectly filtered water are obtained from forty acres of ground, and the appearance of the water is not affected by the turbidity of the river. A suggestion almost inevitably arises, that if artificial filters require frequent cleansing, it may happen that a natural filter, treated thus artificially, may in time become foul. But the assurance is given that "this will certainly not take place for many years." The softening process effected by the use of lime is spoken of in these lectures as being excellently adapted to well water, but as being less easily applied to Thames water, as the suspended clayey matters prevent the carbonate of lime from settling completely, and a filtration is therefore rendered necessary, which offers more difficulties than the ordinary filtration, as the particles of carbonate of lime penetrate far into the sand and soon choke the filter. The Royal Commission on Water Supply in 1868 came to the conclusion that softening by the use of lime was not applicable to the Thames water.

We may add that the Chemical Commission of 1851, while recommending that Clark's process should be applied to chalk waters, went on to say: "But it seems that it is not to river waters that this elegant and useful purifying process is most advantageously applicable." The proposal of the commission was that chalk waters, softened by this process, should be resorted to for the supply of the metropolis. The Royal Commission of 1868 did not take up this recommendation, but relied on proper filtration and due care in excluding polluting matters from the river.

The examination of water, with a view to detecting its impurities, affords a peculiarly interesting subject at the present time. While exaggerated fears are being dispelled, more effective precautions are likely to be taken against the inroads of disease. Already we are provided with the means of testing the efficiency of filtration, so as to be aware when a filter has lost its purifying power. A welcome discovery is furnished by the fact that micro-organisms are largely eliminated by the action of such filter-beds as appertain to the works of the London Water Companies. In testing the results of this filtering process, the impossibility of obtaining water absolutely free from the presence of microphytes has to be recognized. It was stated at Chatham that "all natural waters contain micro-organisms." There is no reason to be alarmed at the fact, for it is added—"by far the greater number of these are of a perfectly harmless character." This circumstance complicates the question, but at the same time it lessens the apprehension that would otherwise arise. The biological examination of water which is now coming into vogue, consists in discovering the relative abundance of these micro-organisms, and in ascertaining, if possible, whether any of them are dangerous to health. Tested by modern processes, even the morning due is not pure, nor the hoar frost that sparkles on the leafless bough. The rain is somewhat better, but all are touched by impurity of some kind or other, gaseous if not animal. A table given in one of the Chatham lectures shows that the average of dew and hoar frost contains more than twice as much organic carbon as the West Middlesex supply, and three times as much organic nitrogen. The total combined nitrogen is less than in the average of London water, and rain is purer than dew, but with more organic carbon and nitrogen than occurs in the New River supply. Rain serves to wash the atmosphere, but due and hoar frost are formed in the lowest stratum, where the impurity is greatest. Even if we resort to distilled water, we there encounter gaseous pollution. By "pure water," therefore, for all practical purposes we

should understand not that which the chemist calls pure, but that which contains nothing prejudicial to health in the case of a drinking supply, or nothing calculated to destroy the success of a technical operation when the water is intended for industrial purposes. If all the microphytes are to be looked upon with suspicion, we must even view the supply furnished by the Kent Waterworks as of dubious quality. Not only are micro-organisms discovered in the Kent supply as it reaches the consumer, but they exist in the water as it is pumped up from the deep wells in the chalk. Strange to say, the well often contains more of these organisms than the supply. The Kent water is not filtered, and hence there is no straining out of the microphytes, which probably multiply after a brief stimulus from the light received on their way from the well to the mains. Filtration has an immense effect on the river waters, taking out from 94 to 99 per cent. of the organisms. Last month a sample of water taken from the Thames at Hampton, and treated by the Koch process, yielded 11,415 "colonies," indicating so many original organisms, per cubic centimetre. A sample of West Middlesex water taken one day later, and treated in a similar manner, yielded only 175 "colonies" per cubic centimetre. In January the supply from the Lea exhibited a yet more striking reduction.

The extent to which the water supply may become the vehicle of zymotic disease is a doctrine now brought into much narrower limits than were formerly understood. It is found that most of the substances ordinarily present in water, and which, chemically speaking, are impurities, have little or no influence upon health. Danger has to be sought in matter of an organic nature. But it is not all organic matter in water that is to be considered dangerous. The evil has to be sought in organic matter arising from animal excreta. The circle has to be drawn yet narrower, for sewage-contaminated water is daily consumed by multitudes of people without any prejudicial effect. The peril presents itself when the sewage is derived from persons who are suffering from disease. The argument put forth at Chatham on this point is that sewage may at any time become infected, and consequently all water which "has been" contaminated with sewage should be condemned for drinking purposes. In its logical severity this rule seems scarcely applicable. In fact, we have hardly reached the end of the story yet. It is said that the infectious properties of sewage may take effect by means of quantities excessively minute. The exciting cause of infectious disease has been shown to depend on the presence of minute living organisms, similar in their appearance and nature to those which have long been known as the cause of fermentation and decay. In the case of typhoid fever and cholera the action of these organisms has been identified with a fair approximation to certainty. The chemical treatment of this question depends on the circumstance that the micro-organisms have an animal origin. As nitrogen, generally speaking, is more abundant in animal substances than in those of a vegetable nature, it may be considered that when the organic matter in water possesses a large amount of nitrogen in comparison with the carbon, this organic matter is of animal origin. If, at the same time, there is an undue proportion of ammonia and nitric acid, the state of the water becomes still more suspicious. Still, as already indicated, the case is simply one of suspicion, and the question yet remains whether the organic matter is accompanied by the presence of zymotic germs. The argument has much force, that towns ought not to be supplied with water which is subject to animal contamination, seeing that such a supply is liable to become infected with the virus of disease. Suppose the zymotic germs to enter the Thames from a house-boat on the river above the London intakes. Is the metropolis thereby threatened with sickness and death? Theoretically it is so. Practically it may be otherwise. Is all London really at the mercy of a single cluster of germs? Will nothing kill these creatures or impair their vitality? Can they not be arrested on their way, and so be turned aside from their purpose? We are thankful for the statement made at Chatham, that recent investigations carried out by the aid of the gelatine process "have brought to light the remarkable fact that these micro-organisms are by no means so unimpres-

sionable as had previously been imagined." We have seen that filtration arrests them to such an extent that only about one in fifty may be said to escape. If Dr. Koch, or Dr. Percy Frankland, or Dr. Dupré—who has a method of his own—can manage to distinguish between the harmless and the hurtful of these microphytes, positive knowledge of the most valuable kind will be thereby placed at our disposal. Until then we are still in a great measure groping in the dark, though there is a glimmering light ahead which promises to lead us in the right direction. That which we may properly demand is not the unattainable, namely, water that is absolutely pure, but that which is reasonable, or in other words, water which is practically wholesome. It is to the biological examination of water that we have to look for a determination of this question, and we rejoice to find at the close of the Chatham lectures a belief expressed that this new mode of investigating the problem of the water supply is fraught with the probabilities of success.

BLOWHOLES IN OPEN-HEARTH STEEL.

By JOHN HEAD.

Paper read before the Iron and Steel Institute.

Although mild steel is gradually displacing the better qualities of iron in the construction of boilers, ships, and bridges, its behavior in certain cases is somewhat puzzling, and has sometimes given rise to much anxiety, fractures having occurred which, up to the present time, have been classed as of a mysterious character. It is the object of this paper to point out one of the defects to which these mysterious fractures may be attributed, and, at the same time, to show how that defect may be avoided in the manufacture of steel. It is fortunate for steel manufacturers that Mr. W. Parker, the chief engineer surveyor of Lloyd's, has not allowed himself to be frightened by the mysterious failures which have occurred in the employment of steel, but, on the contrary, has made searching inquiries in each case brought under his notice; and manufacturers are much indebted to him, and to Mr. J. T. Milton, and to Mr. Stromeyer, also of Lloyd's, for the valuable reports which they have drawn up, and the papers they have written on the subject, copies of some of which were kindly placed at the author's disposal before he undertook to write this paper. Those reports and papers treat of various matters in connection with the employment of steel for the construction of ships, boilers, and engines, and they lay great stress on the danger of working steel at what is termed a blue heat, that is, a heat ranging between a straw color and blue. It would be needless for the author to insist further on that important point, which has been so ably dealt with at the meetings of different institutions, and quite recently at the Institution of Civil Engineers on the reading of a paper by Mr. Stromeyer on the subject. It is quite another matter the author wishes to deal with in this paper, viz., the presence of blowholes in open-hearth steel, by which the strength and reliability of that metal are affected, and the means to be adopted for avoiding them. Blowholes in steel are of two kinds—those due to the contraction of the metal in cooling, and those due to the presence of imprisoned gases in its mass. With care and by the use of proper appliances in casting, the first kind of blowholes may in a great measure be avoided; but, even when present in steel ingots, inasmuch as they form vacuous cavities, the author agrees with the generally accepted theory that they will close upon the steel being subjected to pressure under hammers, presses or mills, provided that air does not reach them before the weld is complete. But with regard to the second kind of blowholes, the case is different; they can only be altered in form, except in so far as they may be got

rid of by being brought to the surface of the metal, by compression while the ingots are being worked up into merchantable form. Supposing an ingot of steel containing gaseous blowholes to be placed under the hammer or mill, the effect of the work done will be to compress the blowholes, except in so far as some of them may be brought to the surface and so be removed; but the others will remain in the finished article, forming points of weakness which will show themselves on testing for tensile strain, particularly when the strain is applied transversely to the direction in which the metal has been worked. In the removal of blowholes—those of the first kind by welding, and those of the second kind by bringing them to the surface of the metal, in working it up into merchantable forms—is to be attributed the improvement in quality which steel is found to acquire by compression after the ingot has been withdrawn from the mould.

It will be understood that the blowholes due to imprisoned gases in steel are in most cases extremely numerous, although minute in size, and they are not uniformly distributed in the ingots. They are similar to the cavities in glass, technically known as seedy boil, which may be seen in the sample bottles, &c., which have been placed on the table, marked as made before the year 1883. Later on reference will again have to be made to glass and its manufacture, as affording a ready means of explaining the manner in which blowholes in steel are produced and may be prevented. During pouring of steel the gases it contains rise to the top of the ingot or casting so long as it is in a sufficiently liquid condition for that purpose. Such upward movement is soon arrested, however, by the metal assuming a plastic condition, but at the top there will always be found a greater number of blowholes than at the bottom, and these will be much larger, as there is little head of metal at that point for compressing them. These blowholes cause the rising or boiling action which has necessitated stoppering moulds with sand and wedge plates; whereas the metal, shrinking on cooling, should, on the contrary, cause the top of the ingot to be depressed, or to take a cup shape. To the presence of gaseous blowholes in steel in greater or less number or size is due the difference in tensile strain which test pieces from the same charge, and frequently from the same plate or bar, withstand. Although this circumstance has not hitherto been remarked upon, it has been admitted and allowed for in practice, for the Admiralty, the Board of Trade, and Lloyd's rules all allow a margin of 4 tons, between 26 and 30 tons, in the tensile strain of boiler plates, per square inch of sectional area, while for ships' plates, Lloyd's rules also allow the same margin of 4 tons between 28 and 32 tons, which represents a range varying to the extent of one-seventh on the minimum strength insisted upon. In his paper, read at the Institute of Naval Architects in March of last year, Mr. Parker said: "This range of about 4 tons in the tensile strength of a plate of homogeneous metal like mild steel is very unsatisfactory." This is the only comment which appears to have been made on this interesting subject.

In explanation of some of the failures in steel which have hitherto been classed as mysterious—no attempts at explaining them having been made—the author would suggest the possibility that in certain cases the gaseous blowholes in an ingot may have sorted or arranged themselves in a series, thus forming a line of weakness in the plate or bar which has failed along that line when subjected to a strain much below that which test-pieces from the same plate or bar would withstand. The failure of the boiler plates described in Mr. Parker's paper previously referred to, read at the Institution of Naval Architects, might be thus explained; and Mr. Cuthill's remarks during the discussion which followed, seem to confirm that conclusion, for he said: "Some of the tests since made of the plate corroborate the original, while others are considerably higher, averaging about 2 tons over it. No explanation can be given as to this, other than that the original was taken from the scrap edges, while the higher ones and better in extension, have been taken from the plate." Other failures in steel have occurred while the metal was being worked up into ships or boilers, which may also be explained by the existence of blowholes, forming a line of weakness in the plates, but not necessarily a straight line.

When the plates were riveted at the ends of the line of blowholes, the tensile strength would not be much affected; but when riveted at the sides, crossways, there would be a certain stress produced which would lead to breaking. These failures are apparently complicated by the circumstance that they occurred when the men were away from their work, either during the dinner hour or at night, but on consideration it will be seen that this circumstance will materially assist in arriving at an explanation, for the rivets, when put in, were in a heated condition, and heated the plate to some extent, thereby causing a certain amount of expansion, which was sufficient to allow for the stress; but on the men ceasing work, cooling took place, stress was put upon the plate, and it snapped across in the line of blowholes, but not necessarily through any rivet-holes.

Speaking of the fractures in the steel plates supplied for the boilers of the Russian yacht Livadia, in their report dated February 16, 1881, Mr. W. Parker and Mr. J. T. Milton say: "It was also suggested that the brittleness might have been induced by the absorption of gases." It is to be regretted that this failure was not inquired into from the point of view here indicated, as such an inquiry could not have failed to elicit valuable information. At some works agitating the molten metal in the ladle before pouring into the moulds is practised for the removal of gases from Bessemer steel. To our past-president, Sir Henry Bessemer, is due the honor of having first suggested that means; and in the year 1881 Mr. W. Allen brought before the meeting of this institute a mechanical agitator which he had used with advantage. In the discussion which followed the reading of Mr. Allen's paper, the late Sir William Siemens spoke of some trials which he had made for poling molten steel in the ladle, in order to remove the gases it contained; but he was evidently aware that all these means were expedients, designed to mitigate a defect in steel manufacture which could not at that time be removed, for he added a remark I shall quote at length later on as bearing pointedly on the subject under consideration. It may be added that at a works where the manufacture of both Bessemer and open-hearth steel is carried on, the latter in furnaces heated by radiation from flame, an agitator is used for Bessemer steel, but is not required for open-hearth steel, which lies quite still in the moulds without having recourse to that means.

It has been supposed that the gases forming blowholes in steel are bubbles of carbonic oxide formed by the reaction of iron oxide on the carbon in the manganiferous iron, added to the molten steel before it is poured into the moulds. This explanation, however, is open to the objection that manganiferous iron, being specifically heavier than steel, sinks immediately to the bottom of the molten metal to which it is added, being only momentarily in contact with the slag on the surface; and, further, that, as already pointed out, we find gaseous blowholes, or seedy boil, in glass, in the manufacture of which the addition of manganiferous iron, or any material containing carbon, at the end of the operation is not practised. If the gaseous blowholes found in open-hearth steel are bubbles of carbonic oxide, hydrogen or air, these gases may have been obtained from those used for melting; and this conclusion is supported by the fact that blowholes or seedy boil are found in glass and steel melted by contact of flame, and are absent when melting without contact of flame is adopted. Diagrams are given, representing, respectively, longitudinal and transverse sections of a plate-glass melting furnace heated by contact of flame—such furnaces as were constructed by Messrs. Sir W. and F. Siemens about the year 1862. In this furnace the flame enters the heating chamber at the level of the siege, and plays around and on the top of the pots, so as to obtain as much contact as possible between the flame and the materials to be heated; after doing its work in the heating-chamber the flame leaves at the end opposite that at which it may happen to enter at the time. In the manufacture of plate-glass in such furnaces, the pots are charged at intervals with the mixture called batch, being filled in the first instance, and refilled each time the materials are melted into glass, and thus diminish considerably in bulk. When the last charge has been melted, the greatest heat possible is put on for about

an hour in order to heat the furnace and its contents thoroughly, after which gas is entirely, or nearly entirely, cut off, and the furnace closed, for about four hours, to allow the metal to fine. The fining of metal in this manner is for the purpose of removing seedy boil, or blowholes, the gases forming them rising to the surface during this operation. The fining operation will be completed in about an hour, but as plate-glass is required in a plastic condition, flame is kept out of the furnace for some hours longer before the metal is ready to be cast. The pots themselves, and their contents, are removed for that purpose, if large plates are to be produced; or the metal is ladled out of them when it is only required to produce small articles, such as tiles, ships' lights, &c.

The manufacture of window-glass is carried on in the same manner as plate-glass, so far as regards charging, melting and fining; but as this glass is worked hotter than plate-glass, less time is allowed for cooling before blowing is commenced. Some plate-glass furnaces hold as many as twenty-four and thirty pots, called cisterns, each containing about 12 cwt. of metal, so that each found in the larger furnace will weigh about 18 tons. Window-glass furnaces hold from eight to ten pots, each containing about 3 tons of glass; in bottle-glass furnaces, the pots employed are of smaller capacity than those used for window-glass, each holding about 1¼ ton. In order to remove seedy boil from glass melted by contact of flame, recourse must be had to the method of fining described, that is, the metal must be kept under the influence of heat without contact of flame. The operation will be more or less complete according to the temperature maintained in the furnace after the flame has been cut off, and the time which can be allowed for the glass to settle. In the manufacture of common bottles, cheapness of production rather than good quality of glass, having, until recently, been the object aimed at, the glass made, as shown by the bottles exhibited, contained much seedy boil. In order to reduce the cost of production, and at the same time improve the quality, Mr. Frederick Siemens some years since invented the continuous glass melting process, which is adopted by all the leading bottle-makers in England and on the Continent, and is now being largely introduced for making window-glass. Since the introduction of this process, bottle-glass furnaces have been enormously increased both in their holding and working capacity. Up to 1882 the largest furnace of this kind was one of those built in England, the tank measuring 42 feet long by 16 feet wide, and holding about 150 tons of metal; but in combination with his new method of heating by radiation, Mr. Siemens, at Dresden, has been able to augment considerably the capacity of these furnaces, and a photograph is placed before the meeting of a circular furnace at his works measuring about 40 feet diameter, and holding about 230 tons of glass in fusion.

Diagrams are also given showing, respectively, longitudinal and transverse sections of a tank glass furnace heated by contact of flame, these diagrams being taken from the drawings of a furnace similar to that just referred to as holding 150 tons of glass. The flame is made to strike on to the metal in fusion, with the result that the glass made contained some seedy boil; but the glass produced in this furnace being better than that made in the furnaces which is superseded, no notice was taken of the circumstance at that time. As bottle-glass-blowers commence work each week on Monday morning at six o'clock, and leave off work about the same time on Saturday morning, that day is available for cleaning the flues, during which operation the glass in the tank is partly refined, as already explained, further fining occurring on Sunday, when the furnace, not receiving any addition of fresh batch, gas and air have to be partly cut off, to reduce the heat, and under these conditions the flame would not come into contact with the metal in the tank. Thus a certain amount of refining was always practised in making glass by the continuous melting process, and, when necessary, the tank could be treated as a huge pot, all the heat possible being applied for some time, after which, the furnace doors and openings being closed, the flame would be reduced for a few hours by shutting off nearly all the gas and air. On one occasion the author dealt with a large tank furnace in that manner, and, as was expected, the metal

was found afterwards to be of excellent quality and free from seedy boil, or blowholes. By the adoption of Mr. Siemens' new method of heating furnaces by radiation, the loss of time and fuel consequent on fining when pot furnaces are employed is avoided, and seedy boil has disappeared in glass made by his continuous melting process. The presence of seedy boil in glass is therefore attributable to the contact of flame with materials in fusion, and is avoided when glass is melted by radiation from flame. The same result is obtained when glass is melted in closed pots; this is another but rather expensive way of avoiding contact of flame with materials in fusion, which in glass-making can only be adopted for the best qualities of glass, table glass for instance. Samples of glass melted under these conditions are exhibited, and will be found to be quite free from seedy boil.

A good form of furnace for melting glass continuously is shown by two diagrams, one being a longitudinal section, the other a sectional plan. The gas and air enter into combustion at a certain distance above the metal in the tank, and the roof and walls are so arranged that the flame does not touch them, but sweeps round the heating chamber, in horse-shoe form, to the exit on the same side of the chamber as the admission ports. For the manufacture of glass this form of furnace offers many advantages, as, while providing a long run for flame, thus promoting perfect combustion, it gives a large extent of surface at which the workmen may be placed for gathering and working. This same form of furnace has also been adopted with advantage for melting steel on the open-hearth; or long furnaces, holding 40 to 50 tons of steel are used, in which case they are made fish-bellied in plan, so as to allow space for lateral expansion, as well as a long run for the flame. In Mr. Siemens' furnaces at Dresden, before referred to, the run of flame from the inlet to the outlet ports is nearly 100 feet, and the greatest uniformity of heat is thereby secured, combined with economy in working. It has been shown that seedy boil or gaseous blowholes are invariably found in glass when made in furnaces in which contact of the flame with the materials in fusion occurs, and it has been shown also that these defects may be removed by fining, or may be altogether prevented by melting glass without contact of flame—that is, in closed pots, or by radiant heat. The same remarks hold good with regard to steel. What is wanted at the end of the operation is that the fused metal should lie quiescent for a certain length of time under the influence of intense surface heat without contact of flame. In glass-making, seedy boil may be obviated in two ways—either by fining, as has been explained, or by melting without contact of flame, but for making steel on the open-hearth the latter alternative is alone practicable, for the reason that it is required to be tapped as hot as possible, whereas glass is worked in a semi-plastic condition.

The desirability of maintaining steel in a quiescent state for some time before pouring was recognized by the late Sir William Siemens, who, after describing the trials he had made of poling steel in the ladle, before referred to, made the following prescient remarks: "I have observed that considerable benefit accrues to the steel if, after it has been poured into the ladle, it is allowed to remain there under the protecting covering of slag to prevent decrease of temperature." The objection to this mode of proceeding is that, at this portion of the process, time is valuable; but I feel sure that practical benefit will accrue if the steel can be kept in a quiescent fluid state for a quarter of an hour before pouring. Of course, it would not do to leave steel for a quarter of an hour in the ladle; it would partly set, or become plastic if that were done; and Sir William Siemens referred to that circumstance when speaking of time being valuable at that part of the operation. In 1881 all furnaces were heated by contact of flame, but his genius enabled Sir William Siemens to forecast the requirements of steel manufacture to render the operation perfect. Mr. Frederick Siemens had not then invented his new method of heating by radiation, and had Sir William Siemens been spared to us a few years longer he would have been pleased to find in the application of this invention the solution of the remaining difficulty in steel manufacture.

There is another point to which, before concluding, I wish to draw your at-

tention, at least briefly, viz., the analogy which exists between glass and the slag covering steel in open-hearth furnaces. If the slag were deprived of most of its iron, it would become clear glass, hence the method of fining applied to glass-making will also apply to slag in steel-making. In the early days of the open-hearth steel manufacture, glass was proposed as a protective covering for steel to prevent flame from penetrating to the molten metal beneath, and the slag floating on steel is perhaps even now looked upon as affording such protection. The behavior of glass when melted by contact of flame will not, however, as has been shown, sustain that opinion. In bringing to bear upon the subject of steel-making the results of experience obtained in another manufacture, members will understand that the author is dealing with figures worthy of their attention, the quantities of glass treated in continuous glass-melting furnaces varying between 80 and 230 tons at a time, and so far as the dimensions of these furnaces are concerned, it need only be said that they much exceed anything as yet employed in steel-making. This will become evident when we consider that the tank furnace described, which is one of many built in England, would hold 375 tons of steel, and the furnace described as built at Dresden—Mr. Siemens having several furnaces of the same size in operation—would hold 575 tons of steel, the densities of steel and glass being taken as 7 and 2.8 respectively. There can be no doubt that by manufacturing open-hearth steel free from gaseous blowholes the metal produced will be much stronger and more reliable than that made by contact with flame, and the result will be a greater confidence in its use. The merit and utility of Mr. Siemens' new method of heating by radiation, by which this may be effected are enhanced by the consideration that the production of steel of superior quality by its means is attended with considerable saving in cost of construction, in wear and tear of furnaces, and in waste of metal, the yield of sound bars or plates from a given weight of ingots being considerably more than was attainable with steel melted by contact with flame.

Mr. Aitken said he had given considerable attention to the subject of gases in metals, and had spent many thousands of pounds in experimenting, but had obtained no commercial results beyond a slight increase of knowledge. It occurred to him that the difficulty resulting from gases in steel might be dealt with by mechanical means. No doubt, as the paper pointed out, if iron and steel could be dealt with in the same way as glass was dealt with in Mr. Siemens' furnaces, and kept hot for a long time, the gas in the metal would to a large extent be got rid of, and a sounder ingot would be obtained. Moreover, these metals were very much more expensive than they would be if any practical method of freeing gases from metals by mechanical means could be adopted. Mr. Aitken then referred to a diagram in which he suggested one of the modes by which this object could be attained. They had now taken up the subject again, because he thought the time had now arrived when the subject was rife for discussion. Seven years ago he took out a patent for a mechanical means of freeing gases from metals, but he had been able to do nothing with it, because it seemed that the question was not ripe. The diagram represented a ladle filled with molten metal. The exhaust chamber was lowered from the top and the nozzle projected into the upper part of the metal. The vacuum was then applied, and four tons of metal were drawn up into the vacuum chamber. When Sir Henry Bessemer made some experiments he took a pot of molten steel from the Bessemer converter and poured it into a crucible, and he put that under a vacuum. As a result, when the vacuum was applied the metal swelled and almost exploded, and there was only one-third of it left in the pot. Therefore when the metal was drawn by the vacuum into the vacuum chamber he expected that a very large expansion of metal would take place. When one charge was done the air would be admitted again, and the metal would fall back into the ladle. It was found that metal treated in that way was of somewhat higher specific gravity. Having lost a certain amount of gas the metal was more dense, and the gases in escaping had also absorbed a certain amount of heat. The denser metal would be found to settle at the bottom, and by successive changes it would be possible

to free the whole of the steel in the ladle from the gases. Then the question arose, What would be the effect of that? His own impression was that steel would be very much strengthened by the extraction of the gases, and if this plan were adapted to the Bessemer process he thought the use of spiegeleisen might be done away with.

Mr. Walker regretted that a man like Mr. Head, with his extensive experience of steel, should have put before the institute the words, puzzling and mysterious, in reference to steel. There was no such thing as mystery and no puzzle about it at all. When there was anything wrong with the steel it was because it had been badly manufactured and badly heated. Men accustomed to use both steel and iron knew that there were ten times as many wasters in iron as in steel. If an iron plate had a hole in it that you could put your knife in it was sent back and nothing was said about it, but if the same thing occurred with steel, it would be said there was a mystery as to the cause of the failure. Just as the workmen increased in skill would the number of waster plates decrease. There was no comparison at all between the qualities of steel and iron, because shapes could be moulded in the former metal which could not be made in iron at all. The case of the plate referred to by Mr. Head was not, he thought, due to blowholes, but to want of work. It was not well made. He himself paid particular attention to the matter, and went over to Cammell's at the time. There was no mystery about it, but it was as plain as a pikestaff how the defect came about. With reference to the apparatus which Sir Henry Bessemer had suggested for fusing the metal, its use was quite the exception, and there were very few people who did that. As good steel as he had ever seen had been made in the Bessemer furnace.

Mr. Windsor Richards said he had had a good deal of experience during the last year in making soft steel plates, and had experienced a good deal of difficulty and trouble from two causes, one being the blowholes, and the second and greatest, the irregular distribution of carbon in steel. At Eston they operated on large ingots weighing about 4 tons, 6 feet long, 36 inches wide, and 66 inches thick. Mr. Head had very carefully checked the chemist's analysis, and the carbon in the steel was found to vary from 0.10 to 0.15. The great trouble when ingots had to be rolled was to keep within the limits required by Lloyd's—about 4 tons. Very many things had been tried to get over this serious difficulty, the mechanical agitator, as suggested by Sir Henry Bessemer, and poling the molten lead with a very long pole, as suggested by Sir William Siemens, the latter being more comical than effectual. They also allowed the metal to rest in the convertors for half an hour before pouring, but that did not do, and none of the things they tried were of any avail. At last they tried what had been done at other places, though he thought not for the same reason. Mr. Riley, of the Steel Company of Scotland, poured the metal from one ladle to another, and he thought if he could, after putting the ferro-manganese into the ladle, pour the metal from one ladle to another, he would get rid of the gas, and get a more equal distribution of the carbon. He was glad to say that this method had been very effectual indeed. He would suggest to those experiencing a difficulty to try the process in operation at his own works.

Mr. James Riley said he was very pleased Mr. Richards had made that last statement. He was going to call attention to the fact that steel made in furnaces in which there was contact with the flame was not all of the character described by Mr. Head. Those of the members who last autumn visited the works of the Steel Company of Scotland would, no doubt, remember that the ingots were not stoppered—not at the Newton works, at all events. On any day they pleased they could see the same thing done with regularity and consistency. So dead was the metal, and so free from the gases upon which Mr. Head had dilated, that there was no necessity for the stoppering. That being so, he did not think there was any necessity to found a theory as to mysterious failures and mysterious qualities such as Mr. Head had brought forward, attributing them to them to the presence of blowholes in the steel. He did not believe it for one moment. At the meeting of a kindred institution (the Institution

of Naval Architects) a fortnight or three weeks ago there was something like an apotheosis of steel. There seemed to be a general consensus of opinion that now at last we might take it for granted that we sufficiently understood the metal, and that henceforth there was to be no serious anxiety so long as steelmakers would properly look after their manufacture. It was astonishing, therefore, to come here now and listen to a paper which raised again the bogey of the unsatisfactory nature of steel. Without referring at greater length to that, perhaps he might be allowed to mention a little circumstance which had been known to him for a long time, but which he had not before referred to, because he was rather afraid of communicating too much information, which at times had been turned upon them rather unexpectedly. These mysterious failures were not due to the causes alleged by Mr. Head, but too often they were due, as he thought Mr. Richards was going to explain in his remarks, to the irregularity of the composition of the ingot in different parts. Mr. Richards referred to a variation in carbon amounting to 0.10 or 0.15, but perhaps it might astonish them to know that not infrequently there was a difference of nearly $\frac{1}{2}$ per cent. of carbon in an ingot. It was well enough known to many gentlemen present how that arose. It was a difficulty which had to be met and faced, whether known or unknown to those who were manipulating steel. So surely as that was ignored, so surely were steelmakers landed in difficulties sooner or later with the material they sent out of their works. He believed it was impossible to cast a large mass of steel, so far, at all events, as the heavier ingots were concerned. It was impossible to cast the very heavy ingots which were sometimes made of a uniform character throughout. It was a well-known fact, and had been stated once before, some years ago, at one of the discussions of this institute. But the same thing obtained in the comparatively smaller ingots which they were all dealing with every day. To get over that, to a certain extent he adopted the plan of pouring from one ladle to the other, as Mr. Richards had mentioned, and whenever he called upon his people to make steel of great regularity for certain definite specific purposes, of greater regularity, that was to say, than ordinary. They might depend upon it that if steel would be made where steel could be dealt with, by means of two ladles, a greater uniformity and a better result would invariably be found. This matter of the liquation of carbons and phosphorus and these other component parts was a very serious one, and it was this knowledge which had led him to advise Mr. Parker time after time to keep down the size of the boiler and other plates, and not to let them go to such an extreme as there was a tendency towards. Some might understand why he could not before put this matter forth explicitly. It would be kept back, but he had known of it and carefully guarded against it. This irregularity was mostly found in the upper portions of the ingot. Remove that upper portion of the ingot and get rid of these inequalities, and the material would be very much better than was ordinarily the case.

Mr. Parker said he was very pleased to hear what Mr. Riley had said. They had been endeavoring to discover where plates had failed for a very long time, and he thought this was the first time they had heard a steelmaker speak out frankly. They would all admit—at all events they did at Lloyd's—that the steelmakers should know a great deal more about it than the surveyors, and the latter had done all sorts of things in order to try and draw them to a special declaration. He noticed that his name was associated with the paper, but though that was the case it must not be considered that he agreed in the conclusions come to by Mr. Head, as to the cause of the fractures in steel plates. He would not attempt to deal with his theory as to whether making steel and making glass were analogous; but he would endeavor to deal with the practical investigations which he had made into the cause of the fractures. He would not trouble them with a long history, because he had the pleasure of giving the result of his investigations before a meeting of the Institution of Civil Engineers a few months ago, and it could be found in their *Transactions*. It had been his privilege to investigate every accident of a serious nature that had taken place in marine boiler making, and in every in-

stance the cause had been clearly traced to manipulation and not to any inherent defect in the material itself. Mr. Head had gone back to failures that took place so late as 1881, and these, as I said the other night, were thoroughly cleared up, and they had nothing whatever to do with the blowholes in the castings. As a proof of this he would mention that the plates which gave way were supposed to be brittle, but pieces broken from the exact part that gave way from the exact fracture were found to have the usual amount of ductility. Again, if they took a plate that was rendered brittle by throwing straines upon it, either by shearing it, or punching it, or distorting it, or torturing it in some way or another, and made it red hot and allowed it to cool, the whole of its strength was restored, but he failed to see how making the plate hot and allowing it to cool could take away any blowhole or air bubble. No one regretted Mr. Windsor Richards' difficulty more than he himself did, but he was quite satisfied it did not arise because there were blowholes or seedy boils in the plate. It was his privilege to investigate a plate which Mr. Richards made, and this was the first opportunity he had had of explaining the conclusions he had come to. This plate was fitted into a large marine boiler, and as the boiler was being tested to 180 lbs. pressure, the plate suddenly gave way at 110 lbs. pressure, which was, roughly speaking, one-fourth of its strength. He took that plate out of the boiler, and tested it, and found it had a tensile strength of from 28 to 30 tons, a stretch of 25 per cent. in a length of 8 inches; and it bent double cold, proving clearly, to his mind, that there had been some strain set up in the plate by working it in some way. He pursued the investigation further, and analysed the plate, and it was found to be perfect, so far as the material was concerned. He had no hesitation in saying that the plate, when it left the works of Messrs. Bolckow, Vaughan & Co., was as good a plate as could be made; but still it gave way. Now, on looking at a number of plates at the same works and in other works which his duties required him to visit, he found that steelmakers were in the habit of making very long and very large plates. Some of these plates were 20 feet long, and 4 feet or 5 feet wide, and the majority were rolled to the form of the boiler. In a great number of these plates he noticed that, instead of being perfectly straight and flat, there were little hillocks, showing that the center of the plate was longer than the edge of the plate. In that condition they were sent cold through vertical rolls, and they came out perfectly straight. The inequalities were crushed into the sides of the plates, and that was sufficient to cause them to break even without pressure. That was all he had to say in regard to some of these fractures; but he wished it to be understood that never in his experience had fractures taken place because there had been gas or seedy boils in the plates. In concluding his remarks, he would like to say that the shipowning world generally recognized—though, perhaps, this institute did not know it—that the introduction of steel for marine boiler purposes had been the greatest boon that steamship owners had had for the last fifteen years. It was admitted that the pressures had been increased from 70 lbs. or 80 lbs. per square inch to 150 lbs. or 170 lbs., and it had given to the owners a reduction of 20 per cent. in coal. Without this material they could not have done that with the present form of boiler. Half a million of money had been spent in trying to introduce a different description of boiler in order to get at that pressure within the last ten years, but all attempts had signally failed; but steel had given to engineers a boiler which could be worked at 150 lbs. or 170 lbs., and an engine had been introduced which could economically use that steel. A shipowner could therefore sail his ship at 20 per cent. less cost, so far as coals were concerned than he could ten years ago. To his mind this was the greatest stride that had been made in marine engineering since the introduction of the compound engine, and it was entirely due to steel. There had been little failures and little fractures, and they had endeavored to grapple with and get to the bottom of them. To show to what extent this material had been appreciated and used, he might say that not one out of a hundred boilers that came before them (Lloyd's) was made of iron, and from 1878, when he had the honor of reading his first paper on this subject

before the Institute of Naval Architects, down to the present time, he found that 160,000 tons of this material had been put into boilers for the mercantile marine of this country.

Mr. Martell felt, with Mr. Riley, considerable surprise that at the present time, after the great experience they had had with mild steel, a gentleman intimately connected with the manufacture of it should come before the institute and speak again about the mysterious fractures that were so much referred to in former times. A gentleman connected with the firm of Denny & Co., who had built a great number of ships, stated at the meeting of the Institute of Naval Architects that among the thousands of pieces of mild steel they had used within the last few years in the construction of ships, the failures could almost be counted on the fingers. Who could talk about uncertainty when such regularity as that was ensured? He had no hesitation in saying that when the material was carefully manufactured and passed the required tests it might be used with the greatest confidence. There was no necessity to assign any cause for the "mysteries," because they did not exist nowadays. He hoped the day might very soon arrive when mild steel would entirely supersede iron in the construction of ships for the mercantile marine of this country.

Mr. E. A. Cowper said he quite agreed that very long plates should not be used in marine boilers, because there was a greater chance of variation of quality. If one plate harder than another were placed alongside that other, the strain must come upon the stiffest plate of the two when the ship got upon the rocks. He thought that in Mr. Frederick Siemens' furnace the higher roof allowing the products of combustion to form a flame clear of the roof as far as possible was a most distinct improvement. It saved the furnace, it gave a higher temperature, it saved the fuel, and it gave a better product. There was no occasion to go into a number of theories about blowholes, because if the metal was made hotter and kept quiet, those bubbles would go out.

Mr. Greig said he had made 20,000 boilers, and his experience of steel was that it was twice as good as Low Moor under any circumstances. The cracking was due to the ignorance of the manufacturer, as a general rule. He believed in drilling holes, and it was simply nonsense to say that the stuff would not stand it. He would prefer drilling the plate in order to get to know the material, because if he was taken with his eyes shut through a boiler shop he could tell whether the material was right by hearing the punching going on. That was sufficient to indicate whether there was too much carbon, and whether the plate was too hard or not.

Sir Nathaniel Barnaby said that the question between steel plates and iron plates had been thoroughly settled for a long time. The importance of the paper rather pointed to the controversy between the steelmaker and the forgeman. Those who were concerned in shipbuilding and were interested in the change that was rapidly taking place in the use of castings instead of forgings, had found that a great many difficulties arose from the want of soundness in casting. He had had the advantage of seeing in Mr. Roach's shop in New York a steel shaft which had been made for a ship of war called the Dolphin. It had been hammered down to half its size, but when it was put in the ship and tried it broke. Just in the center of the plate there were two large fissures crossing each other. If it should appear that there was any advantage in the new method by which greater soundness could be procured in the castings, he thought steelmakers would not consult their true interests if they contented themselves with saying that they had had great success in making splendid steel plates for ships and boilers. They ought not to be satisfied until they could make thoroughly sound castings, and when they accomplished that the day of the forgeman would be over, and the sooner the better.

Mr. E. O. Harvey said that during the last few years he had had opportunities of comparing the work of the contact furnace with the radiator furnace. At some works he had had them side by side, and in every case for tinplate metal, for boiler plates, and for castings, Mr. Frederick Siemens' radiator furnace gave distinctly better results. The gases did

not play on to the metal, and were not absorbed, and there seemed to be more diffusion of the heat, which caused the metal to cast sounder in the ingot. It had been said that sufficient heat could not be attained, but his experience was just the opposite. He had always found that when there was not sufficient heat it was due to the bad construction of the furnace. With the new radiation furnaces there had been produced regularly at the works he had been at, eleven, twelve, and thirteen charges of tinplate metal per week, with carbon of from 0.11 to 0.14; and in making steel castings he had been able, in many instances to go sixteen, eighteen, or twenty minutes during the casting operation, and still have no skull in the ladle. That spoke amply for the heat which could be obtained in those furnaces. He had tried Sir William Siemens' suggestion to keep the metal in the ladle some time before casting, and he found that it was cogged down better. He also tried another experiment. Instead of keeping it in the ladle a minute or two before pouring he turned off a certain amount of gas in the furnace, and the result was very similar. It behoved every manufacturer to look into this, and adopt that form of furnace which would give the best soundness. With regard to the absorption of sulphur which sometimes took place from the gases which were passing over the metal in an open-hearth furnace, some time ago he tried experiments in ordinary steels varying from 0.3 to 0.6 of carbon, and there seemed to be more sulphur come out in the ingot than he could calculate in the mixture, even allowing for a certain amount of waste by oxidation which always took place in the Siemens's furnace. In a recent report from Olsberg it was stated as proved in Sweden that the sulphur was appreciably attracted to the metal or absorbed in it by the gas passing over it in an open-hearth furnace when in direct contact with the flame. From a considerable number of experiments there appeared to be an absorption of from 0.015 to 0.025 of sulphur due to that cause entirely, and sometimes as much as 0.3 per cent., but it was said that when three kilogrammes of lime were added to 43 kilogrammes of coal in the gas producers, the absorption of sulphur could not be so traced; it was either very much less or there was none at all. He mentioned this because in a furnace like Mr. Siemens' new form it was very probable that there would not be this absorption of sulphur which had been noticed in the old furnaces where the flame played upon the metal.

The president asked Mr. Ellis if he had tested the merit of his invention for keeping the top of the ingot mold highly heated for a long while, with a view of giving time for a more thorough elimination of the bubbles of gas.

Mr. Ellis said he had found that by using a clay top, and keeping it hot, he could to a great extent keep the metal at the top hot so as to allow the gases to escape and the metal to settle more soundly, but there were several difficulties connected with keeping the clay top sufficiently hot, and by using a different method he could obtain the soundness on the top of the ingot in another way.

Mr. J. Hardisty said that the production of small steel castings was still a somewhat difficult problem, but with large castings there was no difficulty whatever. 0.08 of silicon could be safely put in an ingot without interfering with the ductility of the metal.

Mr. John Head, in reply, said it was evident that the users of steel were far more in love with it than the producers. The users thought there was no fault whatever to be found with it, but makers of steel were conscious that there were little points which might be improved upon. This showed that manufacturers of steel had delivered a constantly improving article to the users. He thought that Sir N. Barnaby could not have read the paper, for it made no allusion whatever to iron. It spoke only of steel and glass, and he had tried to bring his experience in connection with glass to bear on the manufacture of steel. Glass was a transparent article, and enabled them to see what they could not see in steel. He inferred that the improvement in glass made without contact with flame might to some extent be made in steel by using furnaces which prevented the contact of the flame with the ingot. Still, prevention is better than cure, and if they could avoid the blowholes in steel it would be better than merely keeping

away the gases which formed the blow-holes in cooling. In addition to that there was the advantage of preventing the addition of sulphur. It had been generally supposed that for making very mild metal silicon was not a very desirable material, and it was not generally used.

THE EFFLUENT FROM THE BERLIN SEWAGE-IRRIGATION WORKS

By PROF. DR. ALEXANDER MULLER.

From "Gesundheits-Ingenieur."

The surface at present under irrigation comprises two districts north and south of the city, with a total area of about 5,000 hectares (12,355.7 acres). The drainage of these districts is discharged into numerous small streams, all of which are tributaries of the Spree. The drainage of the north-eastern districts passes chiefly into the Wuhle, and that of the north-western area into the Panke, while the southern effluent is nearly all carried into the Havel. Owing to the non-completion of the irrigation-works, and the rapid manner in which the drainage of the town was carried out, the northern area was almost constantly flooded, and speedily got into a very bad and swampy condition. Owing to this the effluent was very impure, and it was feared that, partly by means of the water supply derived from the Spree, and partly by the ice, the inhabitants of Berlin might have the indirect enjoyment of their own sewage. This unfortunate state of things at length induced the Government to undertake searching tests, and to frame stringent enactments. Moreover, some of the adjoining owners of land invoked the aid of the law to defend their interests.

The results are here given of the examination of certain test-samples taken from the Panke in 1883, in order to ascertain to what extent the drainage from the irrigation-works had an injurious effect upon the water of that river for bathing purposes. The samples were three in number, and were taken from various parts of the Panke. The first sample, A, was quite clear and bright, but had a yellowish tinge when seen in a glass vessel; the second, B, was greenish in color; the third sample, C, taken lower down the stream, more resembled sample A. The microscopic examination corresponded with the appearances above noted. A showed, after standing, only a slight brownish sediment, containing desmids and bacilli, together with a little humus matter. B, a copious, rapidly-increasing, light grayish-green sediment, consisting of different species of desmids and infusoria, together with varieties of leptothrix, that is to say, of such organisms as are regularly to be found in polluted waters, towards the close of the period of self-purification. C presented the appearance of a strongly diluted variety of B.

On making a chemical examination of these samples, it was found that A, which represented the river in its natural condition, was fairly pure, and quite fitted for bathing purposes, notwithstanding its somewhat high percentage of humus. Attention is called to the marvelous self-cleansing power which the River Panke possesses; for while at Bernau (where in summer time there is very little water) the smell and appearance is pestilential in the extreme, in a distance of less than two kilometers, after flowing sluggishly through the market-gardens and meadows, it again presents a wholly innocent and unpolluted character.

Sample B bore signs of being an effluent from the subsoil in the last stage of self-purification.

Sample C might be regarded as a passable bathing water. Details of the analyses of the 1883 specimensare given.

It is also pointed out that in periods of drought, while the irrigation goes on at the usual rate, the volume of the water in the river rapidly diminishes, and the relative proportion of river water to effluent undergoes a great change. More-

over, it has been noticed that from time to time large quantities of untreated sewage find their way into the Panke.

The results of a second examination of the water in the year 1884, which are then given, are very similar to those already quoted. Detailed analyses of the original river water, represented by sample A, and of the effluent represented by B, are given as follows:

	Constituents in parts per million.	
	A.	B.
Oxide of iron and alumina...	0.5	2.4
Lime.....	97.5	220.3
Magnesia.....	8.9	18.9
Potash.....	3.4	6.5
Soda.....	14.2	99.3
Chlorine.....	17.5	139.2
Sulphuric acid.....	12.1	75.5
Nitric acid.....	..	63.0
Silicic acid.....	9.0	12.2
Carbonic acid (calculated)....	80.3	114.0
Total.....	243.7	751.3
Deduct for oxygen replaced by chlorine.....	4.0	31.4
Total.....	239.7	719.9

Or by arranging the acids and bases, excluding the indifferent silicic acid and the oxides of iron and illumina, the following distribution may be assumed:

	A.	B.
Silicic acid.....	9.0	12.2
Oxide of iron and aluminium.	0.5	2.4
Carbonate of lime.....	160.4	213.0
Sulphate of lime.....	18.2	128.4
Nitrate of lime.....	..	95.7
Chloride of calcium.....	..	30.0
Carbonate of magnesia.....	18.5	39.5
Chloride of sodium.....	26.8	187.4
Chloride of potassium.....	3.1	10.3
Sulphate of potash.....	2.7	..
Total.....	239.2	718.9

As a general result of the investigations, it is pointed out that the orignal river water is as pure in 1884 as it was in 1883; that the drainage from the irrigation works remains equally unaltered; and that the pollution of the river has been traced over the same area as before; that the clarified effluent from the irrigation works, however, is much inferior in quality to the original river water, and in times of epidemics must be regarded with suspicion.

ON A GEODETIC SURVEY OF THE UNITED STATES.

By C. O. BOUTELLE.

From "Science."

I HAVE been often asked why a geodetic survey and triangulation is the only mode of surveying a large area with precision, and why such slow and tedious methods are requisite for needful accuracy. This paper is an attempt to show in popular language, both the processes themselves and their necessity, as also why Congress should act upon the repeated recommendations of the National Academy, and carry out its views.

To many of the habitual readers of *Science*, this letter will appear to deal with elementary matters which they may be assumed to know. To another large and equally earnest class of readers it may convey useful information. Possibly it may help forward the end sought for, and to this every true lover of science will cry "God speed."

Any survey of a small area, as a farm, plantation, or township, may be made by any of the usual methods adopted in ordinary land-surveying, where the area covered by the survey is treated as a plane surface.

The compass and Gunter's chain of sixty-six feet are the usual surveying instruments in this country. They are liable to serious error. Lack of knowledge of the true local magnetic variation of its secular change from year to year, and of its diurnal change between morning and afternoon, with the always impending possibilities of special local attraction at or near the place surveyed are among the difficulties attending the use of the compass. The chain stretches with use, and changes its length with the seasons and their varying temperatures, and is often carelessly carried by men little accustomed to precise methods.

It is not too much to say that any land worth fifty dollars an acre is too valuable to be surveyed with a compass, and any record of such a survey is likely to become a fruitful source of future litigation. The best of such surveys are but approximations to the truth.

Errors from these approximate measurements are cumulative. When such surveys are extended over large areas, as upon our public lands, serious consequences follow, involving present and future doubt and litigation as to boundaries. This is already apparent in the west. It will become more so in the future as land increases in value.

The necessity for greater precision in original public-land surveys, and for means of ascertaining and checking errors already existing, has been forcibly stated in a report to Congress on the survey of the Territories, by the National Academy of Sciences, in November, 1878, printed in "Misc. doc. No. 5, House of Representatives, 45th Congress, 3d session." The report of the academy, and the very strong letter of Major J. W. Powell, which forms a part of it, fully describe the character and consequences of the errors alluded to. It also sets forth the true remedy as only to be found in a method of survey which should be as nearly infallible as scientific skill and a laborious and careful application of well-known principles could make it.

This method, as practised for two centuries by civilized nations, consists of a system of triangles starting from and proceeding toward certain base lines, measured with every possible care with apparatus specially devised to either entirely eliminate or to reduce to a minimum every source of error, whether physical or mechanical, which might vitiate the resulting length of the measured line, or cast a doubt upon its precision.

Apparatus of this nature is now constructed and used in the U. S. Coast and Geodetic Survey, of such precision that the average probable error of the two primary bases last measured with different apparatus, constructed on different principles, is, roughly, about one twelve-hundred-thousandth part of the lengths of the measured lines.

The exact length of the base being ascertained, and a system of triangles built upon it adapted to and covering the country to be surveyed, the lengths of all the other sides of the triangles in the system are inferred from the familiar theorem that "every triangle has six elements or functions, viz., three sides and three angles, any three of which being known (one being a side), the other unknown elements may be computed" with a degree of precision of the same order as that of the known elements. It is therefore only necessary to measure the angles with the same precision as the base, to insure equally precise results. This is so far attainable, that the latest great primary triangulation of the coast and geodetic survey, enclosed between two measured bases six hundred miles apart, met nearly midway, at a line about twenty-nine miles and a-half long. The computed lengths of the line, from measured bases distant about three hundred miles from either of them, agreed within about five-eighths of an inch.

It follows from the above, that, in any system of triangulation carefully conducted, the relation of every point in the system to every other point may be determined with a degree of precision almost absolute. It renders the position of each apex of a triangle infallible, since its error, if any, can only be detected by application of similar methods of precision, which will themselves be liable to the same sources of error.

Referring to what has been written as to cumulative errors belonging to all ordinary local topographical or other surveys, it is evident that, if these surveys include two or more trigonometrical points within their limits, the inevitable error involved in their methods is checked and corrected as each such point is successively reached. If it is not exactly hit, the local survey is wrong, and must be corrected to meet the triangulation point, which stands as infallible in its assigned position as the Pope claims to be in his.

The triangulation gives the relation of every point in the system to every other point. To apply the data thus obtained to its chief use in the construction of accurate maps, from the local surveys thus checked and corrected, another class of observations and reductions becomes necessary to fit the framework which has

been constructed to its proper place upon the surface of the earth. This, with the triangulation, constitutes what may properly be called geodesy. No better definition of this term can be given than that by the late General R. D. Cutts: "Geodesy, in practice, may be described as a system of the most exact land measurements, extended in the form of a triangulation over a large area, controlled in its relation to the meridian by astronomical azimuths, computed by formulæ based on the dimensions of the [adopted] spheroid, and placed in its true position on the surface of the earth by astronomical latitudes and differences of longitude from an established meridian."

The whole system of triangulation thus combined and co-ordinated, and made to occupy its true position upon the earth's surface, may be compared to a human skeleton. As the skeleton is the framework on which is built and sustained the varied elements of the human body, each fitted to and held in its place by the unyielding structure sustaining it, so the triangulation is the framework on which each varied portion of the earth's surface within its range is also fitted to and held in its true position, and the resulting map becomes an absolutely true topographical picture of the country it purports to represent.

But this is only one, and not the greatest, good represented by a well-executed and complete geodetic survey. Every point of the triangulation is carefully marked above and beneath the surface for reference in future ages. Every recorded distance between any two points thus marked becomes a base line, whose length is known with a degree of precision unattainable by ordinary methods. So, also, is the azimuth or angle with the true meridian made by every such line, thus affording means for ascertaining the local magnetic variation and its yearly change. The recorded and published latitude and longitude of any station will enable future astronomers to find close at hand the means of fixing their precise relations to other and distant observatories. As the country increases in population and wealth, its topographical features change. New towns are built, and new roads and new railroads laid out. New maps will be called for and easily supplied, since the framework of the triangulation, executed half a century before, perhaps, is there, always correct and reliable. As the elevations of all the stations above the mean level of the sea have been determined in the original survey, so, if schemes of drainage are planned to bring swamp lands into use for arable purposes, these differences of level will afford data for obtaining the amount of fall and its proper direction. And so long as the earth and sea maintain their relative positions, so long the beneficent effect of early and exact triangulation will continue to be felt.

This is essentially a national work. It cannot be defined by, or confined within, State boundaries. Whatever views may be held as to local topographical surveys, and who shall execute them, it is evident that the framework on which they are to be built must be independent of political boundaries. The triangle sides leap across bays and lakes, or from mountain to mountain and hill to hill, or they travel "upon stilts" across the level swamps and prairies. Nature only fixes its limits. It is homogeneous and universal by its own conditions of existence. The geodetic survey of all our country is therefore a work eminently proper for the national Government to carry on, leaving the other questions of local topographical surveys for national or State action, or for both combined, as in Massachusetts.

The National Academy of Sciences, which is by law the adviser of Congress and the executive upon scientific matters, has twice, at the call of Congress, advised the early execution of this great work, and that its execution should be intrusted to the Coast and Geodetic Survey, as best fitted in men, means, and training, to carry it on. Lately the need of prompt action in the same direction has been well and strongly set forth by Prof. W. P. Trowbridge, of Columbia College, whose large experience gives weight to his words.

If States whose interests require good maps will join with commercial bodies and scientific men in urging legislation, the plan proposed by the National Academy in 1878, and again in 1884, may be carried out with no duplication of other work, but, on the contrary, with cordial and complete co-ordination with other surveys. The whole country would be benefited thereby to an amount far exceeding the outlay.

EVAPORATION.

By DESMOND FITZGERALD, M. Am. Soc. C. E.

A Paper read before the American Society of Civil Engineers.

The paper gives a description of practical results and a theoretical discussion. It alludes to the many branches of meteorological work, in which experiments must be made to arrive at correct conclusions. The author presents his conclusions as to the natural evaporation from water surfaces. His observations on evaporation at Chestnut Hill Reservoir, Boston, Mass., began in 1876, and were continued until 1882 On examining these experiments, with the idea of compiling a paper, he found that more accurate experiments were necessary, and in 1884 he devised an apparatus to automatically plot a continuous profile of the evaporation on a sheet of paper. A full description of the arrangement of tanks and of the measuring apparatus is given. The experiments were principally made in the Bradlee basin of Chestnut Hill Reservoir, which covers an area of 85 acres, and is about 20 feet deep near the center, where the tank was located. The tank was 10 feet in diameter and 10 feet deep, made of staves of wood, spaced an inch apart, and with a thin copper lining inside. This was in the center of a raft 20×40 feet, the surface of which was about 10 inches above the water. The raft was anchored in the basin so as to float freely and always present its head, which was arranged to break the waves, to the wind. It was expected that this arrangement would keep the water inside the tank at the same temperature with that in the basin, but it sometimes varied considerably, being somewhat hotter in early summer and cooler in autumn. A maximum difference of 10 degrees was observed. Observations were continued during 1885. The temperatures of the air, of the water in the tank and outside, the dew point, the barometer and the force of the wind were also recorded. On at least one day in each month hourly observations were made.

The three important factors in evaporation are, the temperature of the evaporating surface, the force of vapor in the air, and the velocity of the wind. The maximum evaporation occurs on a cool day, which has been preceded by warm weather; the maximum recorded by the author being 0.64 inch, recorded on June 23d, 1885, when the mean temperature of the air was 10 degrees less than on the preceding day. On December 19th, 1885, with the thermometer at 12 degrees F., an evaporation of nearly $\frac{1}{4}$-inch was noticed. As evaporation during the winter months is so often placed at zero, this is worthy of note. The evaporation from a large water surface is nearly the same, day and night; it may be greater on some days from a considerable body of water than from a shallow pool. The author has observed temperatures of the water at the surface of 82.2 degrees and and 86 degrees. A shallow pool loses this heat much sooner than a large body, following the temperature of the air more closely.

During two months the Lawrence basin of the Chestnut Hill Reservoir was shut off from the city supply for the purpose of making direct measurements of the actual evaporation from a large surface. From September 1st to 22d the evaporation from the reservoir was 3.83 inches, and from the tank in the Bradlee basin, 3.87 inches. The evaporation from the tank was therefore taken as representing very closely that of the reservoir. In the winter of 1878-79 the author made experiments on winter evaporation, the general result being 0.02 inch per day for the winter average. Many comparative experiments between snow and ice evaporation showed that ice will lose twice as much weight as snow in the same time. Evaporation from ice is believed to follow the same laws as that from water. The temperature of the surface of ice does not follow that of the air any more than that of water does.

The actual monthly mean values of evaporation observed were submitted to a mathematical computation by Bessel's Circular Function, and the following table, showing the observed results and

the mean curve of evaporation, was presented:

Month.	Observed Evaporation.	Mean Curve by Formula.	Per Cent.
	Inches.	Inches.	
January....	0.90	0.98	2.51
February....	1.20	1.01	2.58
March.....	1.80	1.45	3.71
April.....	3.10	2.39	6.11
May.....	4.61	3.82	9.76
June.....	5.86	5.34	13.65
July	6.28	6.21	15.87
August.....	5.49	5.97	15.26
September..	4.09	4.86	12.42
October.....	2.95	3.47	8.87
November..	1.63	2.24	5.73
December...	1.20	1.38	3.53
Total.....	39.11	39.12	100.00

Total evaporation for a year, 39.11 inches. The computed curve corresponds very closely with the mean temperature curve.

The author then considers other experiments on evaporation. In this country those by Rev. Samuel Williams, in 1772, the Croton Experiments made by B. S. Church, M.Am. Soc. C.E., and those at Boyd's Corner, by J. J. R. Croes, M. Am. Soc. C.E.; in England, those of Mr. Charles Greaves, C.E., Mr. Miller and Mr. G. Dines; in France, those of Mr. A. Salles, and of the engineers of the Ponts et Chaussées; and in Russia, the extensive experiments of Mr. A. Wild, which are discussed by Mr. Stelling.

Without presenting an extended treatise on the theory of evaporation the author describes the principal facts which it is important to understand. In accordance with the dynamical theory of the constitution of bodies, evaporation is constantly taking place at a rate due to the temperature at the surface, and condensation is likewise going on from the existing vapor in the air passing into the water, the difference between the two processes being what we call the rate of evaporation. The capacity of the air to hold vapor varies with the temperature, so that the same amount of vapor in air will have a very different ratio to the total carrying capacity at two different temperatures. This ratio is called the relative humidity. At any given temperature there is a certain amount of vapor which will saturate the air, and any surplus vapor must be condensed. This vapor has, as a gas, a certain pressure, tension or force dependent entirely upon the temperature, and which may be expressed in inches of mercury. Very accurate tables of the maximum force of vapor for different temperatures have been given by M. V. Regnault. It is upon the amount of vapor in the air that the rapidity of evaporation largely depends. The vapor forming at the surface of water is of the maximum force due to the temperature of that surface. The rate of evaporation depends upon the difference between the maximum force of vapor due to the temperature of the water surface and the force of vapor existing in the air, but it bears no relation to the relative humidity of the air. This is because the temperature of the water surface does not follow that of the air even approximately. The practical determination of the force of vapor in the air is generally made by observing the difference between the wet and dry bulb thermometers, but a more trustworthy method is to take a direct observation by means of some condensing apparatus. The principal points to be considered in the study of the vapor of the atmosphere are: 1st. The temperature of the air; 2d. The dew point; 3d. The force of vapor; 4th. The quantity of vapor in, say, a cubic foot of air; 5th. The additional vapor required to saturate a cubic foot of air; 6th. The relative humidity; 7th. The weight of a cubic foot of air at the pressure at the time of observation.

John Dalton was the first to ascertain the true principles of evaporation. The author having been led by his observations to believe that the rate of evaporation was not in exact proportion to $(V-v)$ as generally accepted, made a series of experiments to determine the exact relation. The formula deduced by the author from his experiments is;

$$E=0.014(V-v)+0.0012(V-v)^2.$$

In which E = evaporation in inches per hour; V = maximum force of vapor at temperature of water; v = force of vapor existing in the air.

A number of experiments were made to ascertain the effect of wind, the result

being a modification of the above formula into the following, viz.;

$$E=[0.014(V-v)+0.0012 (V-v)^2](1+0.67w^{\frac{1}{2}})$$

In which w=the force of the wind at the surface of the water.

For an approximate formula, the following convenient forms may be used:

$$E=0.0166(V-v)\left(1+\frac{w}{2}\right) \text{ or}$$

$$E=\frac{(V-v)\left(1+\frac{w}{2}\right)}{60}$$

In the case of his own observations, the author determined by a series of experiments that the velocity of the wind at the surface of the tank was about one-third of that registered by the anemometer, which was located about 30 feet higher.

A series of experiments under the bell of an air-pump was made to determine the effect of barometric pressure, the conclusion being that within the ordinary range of pressure the effect would be hidden by errors of observation; and generally that the influence of the atmosphere on the rate of evaporation is in inverse proportion to its pressure.

In regard to the application of the formula, the author considers that if a sufficient number of accurate observations are made, a very exact result may be reached. The computed result for any given hour is possibly more exact than the observed value.

The author concludes that there is no difference between sun and shade, other things being equal, and that depth has no influence, other than that due to its effect on the temperature of the surface.

THE EFFECT OF FROST ON DIFFERENT ROADWAYS.

From "The Engineer."

THERE is scarcely any condition of weather which has a more injurious effect upon our thoroughfares than frost, especially should it chance to be—as it has been during the winter just passing away—of exceptionally long continuance. Every roadmaker is aware of the risks he runs, under such circumstances, of finding that all his efforts towards perfecting the condition of his roads have been negatived by such an untoward event; and it seems desirable, at a time when so many arguments are being advanced by the advocates of the several systems which are in use among us, to consider which of them is the least liable to be affected by this dreaded enemy. It is as well to use the words "least liable," because all experience goes to show that there is not one of those systems but what is, under certain conditions, open to injurious action upon it by frost. No one could walk the streets of London, for instance, during the late severe weather without noticing that in some way or another one and all of the street pavings, of which this city possesses so many diverse examples, had suffered in some degree from its effects. It will be as well to take the cases categorically, and to make reference first to those macadamized roadways which were the first effort of our progenitors towards improving our means of communication.

Intimately connected with this subject is, of course, that of perfect surface drainage. Frost can have but little effect upon any roadway which, retaining its perfect form, and sound as regards its covering, at once throws off all moisture falling upon it. The basis of its operation is thus lost to the frost, and it must prove altogether inocuous. But this is supposing a condition of things which is impossible of realization. No macadamized roadway can, under the exigencies of traffic, be maintained invariably in the state which alone could insure immunity from the injury under consideration. Inequalities must exist in all such road surfaces, affording a lodgment for water in a greater or less degree, and it matters but little what that degree may be. For it is evident

from the teaching of all experience that the shallower the water film the more quickly is it acted upon and solidified, while the effect in causing injury to the road surface is fully equal to that which may be due to a greater mass of frozen water. In fact, in the case of macadamized roads, it may always be said that the thicker the ice that is formed the less chance there is of the metal being disturbed by traffic while disjointed, so to speak, from the effect of frost. It is therefore often found that roads of this class which are comparatively in a good state of repair, suffer equally, if not even in a greater relative degree, than those which have developed more the results of wear and tear. It is on a form of road construction composed of a mass of units liable to disintegration that frost exercises its most potent effects. The swelling of the innumerable joints inseparable from Macadam's system quickly forces out of position the metal, however thoroughly it may have been consolidated by steam rolling or by a constant stream of traffic. The binding material itself assumes a condition in which it readily pulverizes to mere dust, and as the result we see the roadway under frost covered with loose metal, and each passing vehicle cutting deeper and deeper into the disturbed mass. Such an effect, it would seem, can never be guarded against on macadamized roads. Their construction is of a character which lends itself greatly to aid in the destructive effect of frost. Nor is it alone the center of the roadway—that is, the portion which bears the traffic—that suffers. Injury is particularly liable to occur from the cause named at the edges of the road, unless the side drains where they line the metalling make an almost watertight joint with it. Indeed, it is in such position that frost often exercises its most injurious action. At such points the metalling is, as a rule, greatly reduced in thickness, and although it is less exposed to the disrupting effect of traffic it is more easily permeated by damp from imperfectly cleared side drains, and the thin coat of metal, when that dampness becomes solidified by frost, breaks up almost at once, the *debris* being thrown into the side drains, causing a further blocking of them and intensifying the means of injury. So serious are the results in such instances, that a gentleman well practised in road construction held that it would prove economical to combine the system of asphalting with that of Macadam. He proposed to lay a width of at least a foot along both sides of his roads with metal bedded in asphalt or other waterproof material. But there arose the difficulty that always exists in a combination of systems, that in this case being the impossibility of binding the asphalted line with that of pure macadam work. Weakness at the line of junction would have been sure to show itself as the degree of expansion and contraction under varying sorts of weather differed, and the plan—effective enough, perhaps, had it stood *per se*—had to be given up. No amount of precaution that can be exercised with a due regard to economy can, it appears, suffice to guard against the contingency named, or generally to protect the surface of macadamized roads from the injurious effects of frosty weather.

The conditions under which asphalte is employed as a road covering render it much less liable than is macadam to such effects. An asphalte pavement, when perfect, or even comparatively perfect, may be said to be able to resist almost any amount of frost. Consideration of the effect of it in rendering the foothold insecure must of course be left out of sight in the matter under present discussion. If the pavement be so far in form as to insure a free discharge of moisture into the gullies (there being no side drains to keep clear of water accumulation), asphalte will undoubtedly remain unaffected by weather which would seriously try macadam. But it suffers under one disadvantage to which the latter system is certainly not exposed. It possesses little or no degree of elasticity. When, therefore, there is a force operating to disruption, and assuming this to have obtained a fulcrum for action, there is not with asphalt, as there is with macadam, a function enabling it for a while to resist that force. Supposing this to be sufficiently intensified, the effect is immediate and the injury widespread. The latter must go on extending so long as the frost continues or preventive measures are delayed, and the result is far more ruinous than in the case of metalled roads. Now it is certain that there do occur repeated in-

stances where frost has obtained the fulcrum referred to. Asphalt paving much worn by traffic becomes dangerously thin upon its concrete bed, eventually cracks, and then admits water between it and its foundation. Frost then occurring causes the permeated moisture to swell, and upheaval of the exposed edges follows; further sub-permeation then takes place, and the asphalt comes up in large masses. Nothing, it may be conceived, can guard against such contingency if the pavement is once allowed to become so thin as to be liable to crack under weight. Asphalt is particularly free from any such tendency at a moderate degree of temperature; but under a low range of the thermometer it becomes exceedingly brittle, and therefore particularly liable to such an accident. It has been suggested that the very hardness of the asphalt used in street paving is opposed to its economical employment. Injury to this material is almost unknown during the warm summer months, when it possesses a certain modicum of that elasticity which, as has been said, is wanting to it under the wider range of conditions. The question, as it has been put, is whether it would not be possible by admixture to give it a degree of that quality which, while tending to preserve it during frosty weather, would not adversely affect its traffic-bearing properties. Is it, it is asked, wise to use an entirely unyielding material to carry road traffic? There is an analogy, to a certain extent, between an asphalt roadway and the solid concrete roof coverings used in eastern countries. The more solid and unyielding the character of those coverings, the more certain are they to crack from the contraction and expansion, and consequent movement of the beams which support them. This difficulty appears to have been overcome by those dealing with such roofs by filling in the lines of cleavage where they have showed themselves by a mixture of tar and tallow. Those lines of cleavage once established, rarely diversify themselves, and the elastic character of the filling-in material employed enables them to widen or close up without further injury or without admitting water. It cannot, of course, be said that such a method is applicable to roadways; but it may be admitted that it at least proves the danger of using inelastic material under circumstances which must expose it to disrupting influences. Every roadway is liable to these, and if a mixture could be made which would preserve to asphalt when exposed to low temperature even the small degree of elasticity it possesses at a higher range, it is to be believed that much of the danger now due to the action of frost upon it when used for road surfaces would be done away with. Whether this is possible to be done without militating against the traffic-bearing qualities of hard asphalt, present experience does not prove, but experiments in this direction would not be thrown away.

Wood paving combines in a great degree certain of the peculiarities of both the two systems previously considered. As is macadam, so is wood paving an aggregation of units liable to the same disturbing influences, though in lesser degree, while, like asphalt, it has no perfect unition with its foundation, and is therefore similarly liable to the effect of damp getting below it and being acted upon by frost; but it possesses in an eminent degree the quality of elasticity named as being wholly absent in asphalt paving, while the lesser number of joints as compared with macadam enable them to be treated with a material of a more waterproofing character than is possible with the last-named form of work. But then, on the other hand, the main material employed is essentially porous, and in its untreated condition readily admits of the permeation of damp. Of late years, it may be observed, there has been a tendency to lay such roadways with wood blocks wholly unsubjected to any process of waterproofing. This may be economical in first cost, but it must prove fatal to that quality in the long run. Frequenters of Fleet Street would have probably observed the results to such a method which were apparent in the case of portions of its roadway laid not long since with uncreosoted wood. Under the influence of the frost, the whole roadway in places was upheaved bodily, with what after-results in a financial sense can be easily imagined. In a climate such as ours it must without doubt be one of the primary considerations in laying down any road paving that it should be able to withstand in the fullest

possible manner the effect of our constant rainfall. The practice above referred to is in direct negative of such a manifest precaution. To neglect this is to co-operate with the frost in performing its destructive work; and no idea of saving in first cost should be allowed to induce neglect of guarding against after injury. But there is another point of view which has also lately received strong illustration, from which the effect of frost on wood paving may be considered. The elasticity of this system has before been referred to, but it is, as at present treated, limited to the wood portion of it only. The joints, as now filled in, are eminently inelastic, the asphalt employed being as deficient, of course, in that quality as has been pointed out in relation to paving wholly composed of it. It is certain that wood blocks, especially if uncreosoted, or untreated by some other method of waterproofing, must constantly vary, both in shape and size, during changes of weather. But the movement so caused cannot be followed by the rigid material employed for filling the joints. Hence it must follow that at certain periods there must be found interstices through which damp may percolate from the surface between the blocks and their bed. The arguments advanced in favor of elasticizing—to coin a word—this material have, therefore, additional strength in this relation. Intimately associated with this particular phase of the subject is the question of accurate laying of wood blocks so as to insure perfect uniformity in the width of the joints. If this varies, the causes above named operate with increased strength; the blocks become loosened, shift position, and lose all the quality of bonding with those adjacent, which so greatly adds to the solidity of a pavement. An instance in which the neglect of this common-sense precaution is strongly evident is noticeable in Pall Mall. Cheapness seems to have been the only thought with those responsible for the laying of the wooden roadway in that thoroughfare. The blocks were literally almost *thrown* together, and the result after the late frost should prove a caution for the future against any repetition of such false economy in work of this description. It is not too much to say that the state of the Pall Mall pavement is a disgrace to a city like London. Its center was not many months back, after but a very brief life, taken up and relaid; but the portions of it on either side are really dangerous to traffic, as is almost daily proved by the accidents which occur upon it. That much of its present condition is due to the exceptional opportunity its bad laying offered to the action of recent frosts upon the damp accumulated below it cannot be gainsaid. A wood pavement to resist that action must consist primarily of blocks through which damp cannot penetrate. Secondly, its joints must be accurately laid, and of as little width as may be possible consistently with allowing some expansion of the blocks without producing a disrupting effect; and, thirdly, if it be possible to do so, these joints should be filled with material of a less rigid character than hard asphalte. Elasticity is a condition to the long life of a railroad track, as well as to that of the rolling stock. Equally desirable is it that the same quality should, as far as possible, be secured for ordinary roadways.

There remains but one system of construction to be dealt with—that of paving formed of stone pitchers. As regards the liability of it to injury by frost, it combines the weaknesses of both macadam and wood paving, without the advantages in other respects which are patent in both of those systems. However carefully laid, it can never be said to approach the condition of being perfectly watertight. The grouting it receives, especially as this is commonly applied, certainly never insures that quality, and although the blocks themselves, and consequently the major portion of the road surface, do possess it, the joints may be said always more or less to leak, and the result under frost must follow the rules above quoted. This form of paving is exposed to yet another disability almost peculiar to itself. The grouting under the effects of frost becomes exceedingly friable. With the best workmanship employed and the best material, there must yet always be a certain degree of movement in the blocks under heavy or rapid traffic. When the filling-in material is in the state described this motion acts upon it most destructively, and the defect nullifies the one advantage possessed by stone, that it does not con-

tract or expand appreciably under varying temperatures or climatic conditions. Perhaps to that advantage may be added one other, that of weight, as opposing itself to the swelling action of frost, but it must be manifest that it is only when this last is of a very restricted power that such a qualification can be said to possess any advantage.

To sum up the result of what has been written, it may be concluded that all the various forms of paving roadways as at present practised offer themselves with individual peculiarities to the destructive effects of frost. As immunity from percolation between the material and its bed is the chief safeguard from these, the asphalt paving may certainly be placed first in its qualifications for resisting them, though it has been pointed out that this pavement is not free from a particular danger. The three other systems seem to rank pretty much on a parity; but while the case of macadam seems to be hopeless for improvement, it is certain that in that of wood, at all events, more careful laying and the use of some more elastic material for filling in the joints, would go a long way towards enabling it to resist the action of our severe winter frosts.

STANDARDS OF WHITE LIGHT.*

From "Nature."

THE experimental work of the Committee during the past year has not been extensive, as they had no funds at their disposal for experimental research, and they have been chiefly occupied with reviewing what has been done in the past and laying plans for future operations.

Lord Rayleigh has constructed an instrument which he calls a monochromatic telescope, by means of which the illuminated screens of a photometer may be examined, allowing light only of one definite color to pass. It was hoped by Lord Rayleigh that experiment might show that, with some suitably chosen color, this instrument, used with any ordinary photometer, would, in comparing lights of different intensities and temperatures, give to each a candle power which would be sufficiently accurate to represent for commercial purposes the intensity of the light. The Secretary has made some experiments at the Society of Arts, where he was kindly permitted to use the secondary batteries and glow-lamps; but the results so far are not definite enough to justify their publication.

Mr. Vernon Harcourt has been engaged on an investigation on the barometrical correction to his pentane standard, and on another concerning the possibility of using lamp-shades as a protection from air-currents. His researches are communicated independently to the meeting.

Capt. Abney and Col. Festing have continued their observations on the intensity of radiations of different wavelengths from incandescent carbon and platinum filaments at different temperatures, which will go far to assist the committee in their work.

Other isolated experiments have been made by members of the committee, which will be published in due course.

Most of the members have examined the experiments of the Trinity Board at the South Foreland.

Existing Standards.—A consideration of existing standards convinces the committee that the standard candle, as defined by act of Parliament, is not in any sense of the word a standard. The French "bec Carcel" is also liable to variations; and with regard to the molten platinum standard of Violle, it seems that the difficulty of applying it is so great as to render its general adoption almost impossible.

With regard to the so-called standard

* Report of the Committee, consisting of Prof. G. Forbes, Capt. Abney, Dr. J. Hopkinson, Prof. W. G. Adams, Prof. G. C. Foster, Lord Rayleigh, Mr. Preece, Prof. Schuster, Prof. Dewar, Mr. A. Vernon Harcourt, and Prof. Ayrton, appointed for the purpose of reporting on Standards of White Light. Drawn up by Prof. G. Forbes (Secretary).

candle, the spermaceti employed is not a definite chemical substance, and is mixed with other materials, and the constitution of the wick is not sufficiently well defined. Hence it is notorious that interested parties may prepare candles conforming to the definitions of the act which shall favor either the producer or consumer to a serious extent. In view of these defects of the standard candle, it is a matter of great importance that a standard of light should be chosen which is more certain in its indications.

The Committee have looked into the merits of different proposed standards, and the majority feel satisfied that, for all the present commercial requirements, the pentane standard of Mr. Vernon Harcourt—since it has no wick and consumes a material of definite chemical composition—when properly defined, is an accurate and convenient standard, and gives, more accurately than the so-called standard candle, an illumination equal to that which was intended when the act was framed.

Yet the committee, while desiring to impress the Board of Trade and the public with these views, do not feel inclined at present to recommend the adoption of any standard for universal adoption until, further information on radiation having been obtained from experiment, they may learn whether or not it may be possible to propose an absolute standard, founded, like electrical and other standards, on fundamental units of measurement—a standard which, for these reasons, would be acceptable to all civilized nations. They are, however, inclined to look upon the pentane lamp as an accurate means of obtaining an illumination to replace the so-called standard candle.

Proposed Experimental Researches.—Radiation is measured as a rate of doing work, and consequently radiation might be measured in watts. The illumination (or luminous effect of radiation) depends partly upon the eye, and is a certain function of the total radiation. This function depends upon the wave-length of the radiation, or on the different wave-lengths of which the radiation, if it be compound, is composed. This function of the radiation perceived by the eye is partly subjective, and varies with radiations of different wave-lengths and with different eyes. Thus the illumination cannot, like the radiation, be expressed directly in absolute measurement. But the connection between the illumination and the radiation can be determined from a large number of experiments with a large number of eyes, so as to get the value of the function for the normal human eye. This function, however, is constant only for one source of light, or, it may be, for sources of light of the same temperature. It appears, then, that, in the first instance at least, a standard should be defined as being made of a definite material at a special temperature.

The energy required to produce a certain radiation in the case of a thin filament of carbon or platinum-iridium heated by the passage of an electric current can be easily measured by the ordinary electric methods, and the radiation may be measured by a thermopile or a bolometer, which itself can be standardized by measuring the radiation from a definite surface at 100° C., compared with the same at 0° C. The electric method measures the absorption of energy; the thermopile measures the total radiation. These two are identical if no energy is wasted in convection within the glass bulb of the lamp, by reflection and absorption of the glass, and by conduction from the terminals of the filament. Capt. Abney and Col. Festing have come to the conclusion that there is no sensible loss from these causes. The committee propose to investigate this further. This constitutes a first research.

No research is necessary to prove that with a constant temperature of a given filament the luminosity is proportional to the radiation, because each of these depends only upon the amount of surface of the radiating filament. It will be necessary, however, to examine whether with different filaments it be possible to maintain them at such temperatures as shall make the illumination of each proportional to the radiation. This will be the case if spectrum curves, giving the intensity of radiation in terms of the wave-length when made out for the different sources of light, are of the same form. Thus a second research must be undertaken to discover whether the infinite number of spectrum radiation curves, which can be obtained from a carbon filament by varying the current, are iden-

tical in form when the filament is changed, but the material remains so far as possible of constant composition.

It will be an object for a later research to determine whether, when the radiation spectrum curve of any source of light has been mapped, a similar curve can be found among the infinite number of curves which can be obtained from a single filament.

The next step proposed is to examine a large number of carbon or of platinum-iridium filaments, and to find whether the radiation spectrum curve of different specimens of the same material is identical when the resistance is changed in all to x times the resistance at 0° C. If this law be true, a measurement of the resistance of the filament would be a convenient statement of the nature of the radiation curve. If, then, a number of filaments were thus tested to give the same radiation spectrum curve, their luminosities would in all cases be proportional to their radiations, or (if there be no loss in convection, conduction, absorption and reflection) proportional to the electrical energies consumed.

Thus it might be hoped to establish a standard of white light, and to define it somewhat in the following manner: *A unit of light is obtained from a straight carbon filament, in the direction at right angles to the middle of the filament, when the resistance of the filament is one-half of its resistance at 0° C., and when it consumes 10° C. G. S. units of electrical energy per second.*

Since Mr. Swan has taught us how to make carbon filaments of constant section by passing the material of which they are composed through a die, it is conceivable that another absolute standard should be possible—viz., a carbon filament of circular section, with a surface, say, 1-100th sq. cm., and consuming, say, 10° C. G. S. units of energy per second.

Whether such standards are possible or not depends upon the experiments of the committee. The probability of success is sufficient to render these experiments desirable.

Proposed Later Experimental Researches.—Should these hopes be realized, and an absolute standard of white light thus obtained of a character which would commend it to the civilized world, it would then become an object of the committee to find the ratio of luminosity when the radiation spectrum curve of the standard filament is varied by varying the current, and consequently the resistance of the filament.

Thus, by a large number of subjective experiments on human eyes, a multiplier would be found to express the illumination from the standard lamp, with each degree of resistance of the filament.

A research, previously hinted at, would then be undertaken—viz., to find whether the radiation spectrum curves of all sources of illumination agree with one or other of the curves of the standard filament. It is not improbable that this should be the case except for the high temperature of the electric arc.

Should this be found to be true, then photometry would be very accurate, and the process would be as follows: *Adjust the standard filament until its radiation spectrum curve is similar to that of the light to be compared.* (This would probably be best done by observing the wave-length of the maximum radiation, or by observing equal altitudes on either side of the maximum, the instruments used being a spectroscope and a line thermopile or a bolometer.) The total radiation of each is then measured at equal distances by the thermopile. The resistance of the filament is measured, and its intensity in terms of the unit of white light obtained therefrom by the previous research. The luminosity of the compared source of light is then obtained directly.

The committee desired to be reappointed, and to enable them to carry out the researches indicated they ask for a grant of £30.

EXPERIMENTS of Herr J. Koeing on self-purification of rivers, act on the fact that water possesses the power of rapidly absorbing oxygen when presented to it in the state of spray or a thin layer. The author had an apparatus constructed of finely perforated metal, over which he caused a stream of water to flow, most of it descending in fine rain and being largely in contact with the air. Ordinary potable as well as sewage waters were experimented with, and the results were very satisfactory, the oxygen absorbed being considerable in amount. It is recommended that when sewage water is discharged into rivers such should be chosen as have a long course and a rapid flow, presenting considerable surface to the atmosphere.

THE MECHANICAL ART OF AMERICAN WATCHMAKING.

By Dr. LEONARD WALDO.

From the "Journal of the Society of Arts."

The study of the evolution of a mechanical art in any country is a study in social and political as well as in mechanical science. It often depends for its rapid and progressive growth on the characteristics of the people among whom it is planted. Of no art is this principle more true than of the art of machine watch and clockmaking in the United States. Born among a people who from the beginning of their history had to construct their own devices and machines of all kinds, it has had the same growth and individuality which characterizes the manufacture of locomotives, agricultural implements, or house fittings and decorations. Unlike these classes of manufacture, however, and more analagous to the production of firearms, the clock and watch products are suited to other people and other countries. It is the economic question growing out of these facts which has led to my appearance before you this evening, in the somewhat unique position of a visitor from the United States, explaining to an English audience the conditions of growth in a great industry, in the early development of which England for many years maintained the leading position.

One of the sayings which we hear more frequently in England than in the United States, is that the preliminary training for a great President is to be found in rail-splitting or on the towpath of a canal. The truth underlying this homely remark is, that in the United States the freedom from supervision, the assumption of tasks, the absence of help, the necessity for doing things as quickly as possible, the great variety of work which comes to the American boy early, gives him practice in solving new problems without considering precedents. He is obliged to face new difficulties constantly, and he has no one to appeal to for help.

Throughout the country I think you will often be struck with the general mechanical intelligence of artisans. The plumber is pretty apt to know something of carpentry and metal work; the metal worker can paint, or turn from iron to brass. The lines of caste in the mechanical arts are so loosely drawn, that the artisan of one trade is often found, in dull time, at a "job" in quite another department of labor. This versatility is a characteristic of the native American; it is less true of the Irish or the German emigrant who settles amongst us. The presence of this pure American type, the abundant use of water-power, the impossibility of competition in agriculture with the other sections of the United States, early led to the seclusion of New England as the manufacturing district. The phrase "as inventive as a Connecticut Yankee," came from the fact that in Connecticut and the rest of New England the artisan class grew rapidly in numbers, and protected by wise legislation in regard to the patenting of inventions, every artisan feels there is a possible road to wealth in perfecting the tools he is working with, or the method of production of the thing he is working at. At any rate, patents are cheap and quickly obtained; he will take the risk, he thinks, and work in his own line to better his condition. He constantly sees his fellows transferred from the ranks of workmen to that of employer and manufacturer, and the result is that he cares little for trade practices, for custom, for what is old; he is only anxious to improve the present modes, to attain his own success.

It was a mind of this type which showed itself in the son of a shoemaker (who was also colonel in the State Militia) in the little town of Brunswick, Maine. Born in 1812, Mr. Aaron L. Dennison had the training which might be characterized as Presidential, for he carried a mason's hod at the age of ten, and changed his occupation to that of caring for a herd of cows at eleven. At thirteen he found himself sawing wood and

strengthening his constitution for the cares of later life, and a year or so later he was promoted to that position from which so many bright ideas in life have come, the shoemaker's bench. At eighteen his dissatisfaction with this bench led him to take to another, and we find him apprenticed to a clock and watchmaking firm in his native town, where he stayed till he was of age. He left the town of his birth in 1833 to perfect himself as a journeyman watch and clockmaker, going to Boston, where were then good facilities for learning the higher branches of the horological art. His thoughts soon turned upon the organization of labor in the production of watches looked at as machines for time-keeping, and independent of a value as articles of jewelry or art. It is to be borne in mind that from the beginning of the horological art it had been associated with the workers in the precious metals. The promptness of the driving business life throughout the civilized world was but beginning to be felt in its modern force. Few people cared for the minutes of time in domestic timepieces, still fewer for the seconds. The modern ideas were undeveloped, of synchronized clocks, of observatory time-signals, of swift trains, of banking hours, and stock exchanges in which seconds were of high financial value, of thousands of miles of telegraph wire in the same clock electric circuits once or twice every day, so that Greenwich or Washington time might be furnished with unerring exactitude to railroad superintendent or guard alike. The great public yet were unfamiliar with modern business competition and consequent necessity for extreme uniformity of time in engagement, which is an unconscious accompaniment of the life of to-day. But it was all dawning, and to the mind of Dennison it seemed that timepieces should be made simple in pattern, alike in parts, so that pocket watches could be made in enormous numbers, on principles analogous to those in the manufacture of firearms. The idea haunted him.

He writes:

"The principal thinking up of the matter was done when I was in business at the corner of Bromfield and Washington streets, Boston; and many a night, after I had done a good day's work at the store and a good evening's work at home in repairing watches for personal friends, I used to stroll out upon the common and give my mind full play upon this project. And now as far as I can recollect what my plans then were as to system and methods to be employed, they were identical with those in existence at the principal watch factories at the present time."

In 1840, Mr. Dennison predicted "that within twenty years the manufacture of watches would be reduced to a system as perfect and expeditious as the manufacture of firearms at the Springfield armory." Capital was not forthcoming, however, to what seemed a visionary scheme. It was not until 1849, when a friend, Mr. Edward Howard, approached him for his advice as to the expediency of setting up works for the manufacture of American locomotives, that he had the opportunity of suggesting the probable paying qualities of an establishment for making watches as other machinery was made with interchangeable parts, in large quantities, and with the principles of shop management of mechanicians rather than of the watchmakers. This proposal impressed Mr. Howard more favorably than his own plan for building locomotives. Together they found a capitalist willing to share with them the risks of the adventure. I shall use Mr. Dennison's own words as to what followed the meeting of the three projectors:

"I suggested that the first money spent in the undertaking should be for a tour of observation in the watchmaking districts in England, with the view of ascertaining whether the trade of watchmaking was carried on there on the system represented to me by English workmen I had employed from time to time in repairing. Another object I had in view was to find out the source of supply for the necessary materials, such as enamel for dials, jewels, &c."

Then he goes to England on the proposed mission, and writes in regard to it:

"I found that the matter had been correctly represented, but in carrying out their system one-half the truth had not been told. How that the party setting up as manufacturer of watches bought his Lancashire movements—a conglomeration of rough materials—and gave

them out to A, B, C and D, to have them finished; and how A, B, C and D gave out the different jobs of pivoting certain wheels of the train to E, certain other parts to F, and the fusee cutting to G. Dial making, jeweling, gilding, motioning, &c., to others, down almost the entire length of the alphabet; and how that taking these various pieces of work to outside work people—who, if sober enough to be at their places, were likely to be engaged on someone's work, who had been ahead of them, and how, under such circumstances, he would take the occasion to drop into a 'pub' to drink and gossip, and, perhaps, unfit himself for work the remainder of the day.

"Finding things in this condition, as a matter of course, my theory of Americans not finding any difficulty in competing with the English, especially if the interchangable system and manufacturing in large quantities was adopted, may be accepted as reasonable."

I cannot omit, in this connection, a reference to the contemporary art of the application of machinery to watchmaking in France as well as to England. By reference to documents, now in the Guildhall Library, of the Clockmakers' Company in London, I find that the Clockmakers' Company opposed the erection of works and the granting of a charter to a company, known as the "British Watch and Clockmaking Company. (By Royal Letters Patent.) Capital, £250,000 in 10,000 shares. Deposit, £2 10s. per share." In a report of a committee of the guild, they review the attempts of Mr. Ingold to establish companies for systematizing the production of watches and clocks in Paris in 1835, concerning which the committee says:

"The committee is enabled to state it failed entirely, without realizing anything to the parties who embarked their money on the speculation. It was scarcely two years in existence, for early in 1838, Mr. Ingold was in London, endeavoring to open a door to transfer his unsuccessful project to the shoulders of the British public, but the bait at that time was not alluring, and the project for a season abandoned.

"The Parisian Watchmaking Company having expired, a similar company, with similar pretentions, without the name of Mr. Ingold attached to it, made its appearance at Versailles in the year 1838. By some it is denied that Mr. Ingold belonged to this establishment, but it will require more than simple assertion to make such statement credible. The prospectus of this company, issued in 1838, contains the following: Manufacture of French watches, established at Versailles, under the special protection of the king. The manufactory which, from its commencement, has obtained the powerful patronage of the king, has been in full operation since 21st February, 1838. It already employs more than 200 workmen, attracted from the best workshops of France and foreign countries. It is not a matter of speculation—it is a thing accomplished. A board of manufacturers, including the principal watchmakers of Paris, MM. Lepante, Lessine, Charles Leroy, Robin Mathieu, constantly overlooks the perfection of the works.

"The object of the company is to liberate France from the tribute she pays to a foreign country, and to restore to our commerce a part of the thirty millions at least which are drained yearly to buy more than 120,000 watches from Geneva. The company is already in a position to satisfy all orders for watches from 200 to 600 francs; it can multiply the number of its productions by means of the perfect machines which it possesses. By the aid of those machines the principal parts of a watch are made of uniform size and great quickness; the precise exactness is an invaluable advantage, which alone renders the establishment without a rival of its kind.

"A watchmaker who visited this vaunted establishment in 1839, has given the committee the following particulars: I was introduced into spacious premises; two or three directors received me with much courtesy. I was shown a large room containing machinery for escapement making, &c., but of the two hundred men said to be employed, the number I saw did not exceed six or eight, these were occupied in making watches without the aid of machinery, employing only the tools generally in use. I did not see a single complete watch. The person who introduced me to this establishment called at my house in London in 1840, and told me that it was defunct. It is remarkable that coincident with the periods at which the formidable machin-

ery of these companies ceased to move, Mr. Ingold came to reside in London and renewed his canvass for support to a 'British Watch and Clock Making Company' which, increasing its pretensions, has put forth its title to the trade of the whole world.

"The committee has in vain attempted to discover the names of those 'most experienced makers of watches in London, those men prominent in science, not one of whom has expressed a doubt of the efficacy of the machinery to facilitate the manufacture of watches, or of the fact that work so produced will be incomparably superior to that done in the usual way.' On the contrary, some of our manufacturers and several of our practical and scientific workmen who have seen the machinery, have not only directly denied the possibility of its success, but have rejected advantageous overtures to identify themselves with the company.

"In reference to the objects of the company, and the powers it assumes to possess, the committee, as practical men, nurtured in the watchmaking business, explicitly states its conviction and belief that they are absurdly and fallaciously stated. It has given the company an opportunity to prove before it the powers and applicability of its machines to effect the miracles that have been ascribed to them. At an interview, 22d March, 1843, on its premises at 75 Dean street, Soho, they were then requested, urged, and challenged to exhibit proof of the working of their machinery before your committee. The favor was claimed on behalf of the trade at large. This request was refused upon the most frivolous and contradictory pretexts. The committee advisedly declares its firm persuasion that the company is not in a capacity to make good its assertion upon the subject of letters patent which have been taken out to secure these newly invented machines to the company, the committee would remark that the specifications have not yet been enrolled. It considers the features of the case to present the strongest ground for a petition to Parliament, praying it to refuse its sanction to the doubly concentrated monopoly. The rise and progress of this adventure has been thus far traced, together with the career of its founder; he has failed in carrying out a similar object in Switzerland, in Paris, and in Versailles, and he is now reasserting the same pretentions, the stale romance of twenty years, upon the credulity of the British public.

"It may be asked, if this undertaking be founded on such a visionary basis, why not leave it unnoticed to fall piecemeal into ruin?

"The reply is evident and conclusive. At the same time that its failure is most confidently predicted, the consequences devolved in its destruction are not to be lightly regarded. At a time when the watch trade is languishing in sympathy with every branch of our national industry, the uncontradicted assertions of the company's prospectus have aggravated the depression; and although manufacturers of watches are fearless of competition, and regard as impracticable the success of the company, yet they cannot conceal from themselves the fact that a large capital worked against them, under the protection of monopoly legalized by Act of Parliament, though for a limited period, may severely prejudice their interests, and involve a large body of industrious workmen in privation and distress."

The bill for the formation of the English company was thrown out by the House of Commons, on the 31st of March, 1843.

It has been stated in English horological circles that Mr. Dennison had the benefit of Mr. Ingold's ideas and inventions in the formation of his own company in the United States. I cannot find any evidence that this is a fact; and in this connection, as a contribution to the history of the early American horology, I will give Mr. Dennison's own statement of his knowledge of Mr. Ingold and his schemes.

"I have to say I never heard of Mr. Ingold or his operations until after our factory was built at Roxbury, and were hiring our workpeople. An applicant for a situation, a Frenchman by the name of Boudet, I hired to do the facing of pinions, as that work required some hand manipulation such as no machine which had been thought of at that time could do it so well—so far as I know, the thing has not yet been accomplished. This man Boudet told me that he was a son-in-law of Mr. Ingold, and, I think it fair

to conclude, had worked for him in London; but I have no recollection whether he told me so, or whether he informed me of the connection between himself and Mr. Ingold at the time, or after he had been at work some time. I never saw Mr. Ingold, but I knew of his being about in Switzerland during our residence there. I never knew of his being in America until a long time after we came here (Roxbury) to live. I always had the impression that the British Watch Company obtained an act, as I was told by Boudet, or some one I thought knew, that it cost, to obtain the act, £30,000; and that the machine which he got up for making the plates alone cost £6,000."

The first company formed in the United States was not, financially speaking, a success, neither was the second. There had not then been found the financial ability for the head of such a company, and it was not found until May of 1867, when Mr. Royal E. Robbins bought the factory for $56,000—on which Mr. Dennison, his friends and successors, had expended some $250,000—at an auction sale.

It was one of those happy assumptions by a man who had great capacity and a recognized financial ability, of a great manufacturing interest, to which he proposed to bring the new and needed element of business sagacity. It became the precedent for the formation of other companies, and now, instead of the single company at Waltham, there are in the United States its great and well-managed rival company at Elgin, Illinois, and the smaller companies at Rockford, Aurora, Springfield, Ill., and Springfield, Mass., Nashua, N. Y., Columbus, O., Fredonia, N. Y., and Thomaston, Conn., and I am told there are nine others at the present writing in process of organization. The combined output of these factories, which produce watches of durability and a certain excellence of manufacture, is not far from 4,000 per day, of which the parent company produces about 1,100, with a machine capacity for 500 more per day.

The total population interested in the manufacture and selling of watches and clocks, directly and indirectly, in the United States, is not far from 100,000 people, and the sales of the leading company during the year 1884 amounted to $3,900,000 of products.

To give some idea of the extent of the entire industry in 1880, I quote the following table, made up from the returns of the United States Census for that year, remarking that the industry has very much increased since that date:

TABLE SHOWING THE STATISTICS OF CLOCK AND WATCHMAKING IN THE UNITED STATES IN 1850.

Subjects.	No. of establishments.	Capital.	Average No. of hands employed.			Total paid in wages per year.	Value of materials.	Value of products.
			Males over 16 years.	Females over 16 years.	Children or youths.			
Clocks.....	22	$2,474,800	2,807	630	503	$1,622,693	$1,908,411	$4,110,267
Watches.....	11	4,144,327	2,127	1,219	—	1,712,276	982,224	3,271,244
Watch Cases.....	27	1,584,740	1,418	139	201	976,041	2,812,922	4,589,314
Watch and Clock Materials.....	20	117,550	184	45	49	86,050	130,315	300,195
Totals.....	80	$8,321,517	6,536	2,033	753	$4,397,060	5,833,872	12.271,020

It is not to my purpose to follow out the actual making of individual watches by machinery; that is quite impossible in an hour's paper. The study of a machine for the automatic manufacture of screws alone could occupy the time. The

object is rather to analyze the broad difference in the requirements of the industry when it is transferred from the journeyman's bench to the well-appointed machine shop. I shall draw my illustrations from the great company to which I have already referred, having its location at Waltham, a little town some twenty miles from Boston, in Massachusetts. I do this because this company exhibited a very fine array of its machines at the Inventions Exhibition, and my effort now will be to add to your knowledge there gained, the ideas of policy and management regulating such a great establishment.

A new people is not an art people. It takes age, leisure, and accumulated wealth to give masses of the people artistic ideas. The first test they apply to any new machine is that of utility. In the United States this is a fundamental element of the early success of the watch companies. Automatic machines are very costly. No one can afford to make automatic machines for a thousand watches, or for a hundred thousand. It is only when a simple type can be made and have an enormous sale, that machinery can be extensively applied. In the United States the company of which I speak barely lived until the Civil War broke out. Immediately there was a great demand for watches, strong, serviceable, uniform in quality; there was an opportunity to make a simple type. The buyers were not critical as to finish, if only the watches would run. They did not require art in the case, so long as it was strong. A minute a day was accurate enough for their use, and the art could easily enough produce such timepieces. There was the opportunity. The factory which paid no dividend at the beginning of the war, paid 4 per cent. the first year, 11 per cent. the second, 22 per cent. the third, and two dividends of 60 and 150 per cent. the last year of the war. It would have been impossible to pay any such dividends except that the watches were of simple types—interchangeable parts—screws, plates—hands, wheels, and dials, all manufactured by machinists, running well-made machines, under the supervision of machinist foremen.

The machinery was built and paid for. The great questions of getting homogeneous raw material, of learning the capacity of machines, of determining what kind of labor—whether of boys or girls, or men or women—was most efficient in any given department, had been settled. Through the simple, cheap, but efficient work of the years from 1857 to 1865, the whole art of machinery, as applied to making watches, was being studied by men of the highest mechanical and business ability, and without the slightest regard to any preconceived trade notions or customs or traditions. The great idea was that millions of now civilized people wanted watches, with China, and Japan, and India and Africa yet to be heard from.

To create and to supply any such possible demand would require the same business organization as the supply of cottons or metal ware. There must be an organization reaching from the selection of the raw material to the selling of the product in distant markets. To secure homogeneity of material, *i.e.*, uniform texture of the metals employed, without which machine-made watches would be of temporary value, the most recent advances in physics and chemistry must be made use of. To time the watches properly, not only are the observatory time signals from Harvard University used, but a separate observatory, fitted up with a recording chronograph and transit instrument of a refined construction, has been erected. A dark room, with all the apparatus for experimenting in photographic processes of reproducing dials, &c., is in full operation. Systematic experimenting in tempering steel, in testing metals for their physical properties, is carried on, and some of the most exquisite physical apparatus I have ever seen, I have seen in these measuring-rooms. Those of you who are microscopists, and who know of the extreme accuracy of the small standards of length, made by Professor W. A. Rogers, of Harvard University, need only be told that the most beautiful of his smaller machines was built and used at the Waltham Watch Factory, under the combined direction of himself and the mechanical superintendent.

Given a scientific knowledge of the raw materials, a certainty in their selection, and it becomes possible to reproduce the hand processes of manufactur-

ing the small parts of watches by automatic machines. This requires the fundamental department of the company—a well appointed machine shop, in which their machinery can be constructed and repaired. This machine shop occupies the three stories of a wing 150 feet long. The operatives from this department have the constant supervision of the machines, which are systematically arranged along the three and a-half miles of work benches which extend over the nearly five acres of floor space covered by the main buildings. For driving the machinery throughout the factory, a Corliss engine of 125 horse-power works through 39,000 feet of belting, 10,600 feet of main shafting, 8,000 feet of wall rods, and 4,700 pulleys. The number of operations, most of them accomplished by automatic machinery, which is "tended" by a young woman or man having no special knowl- of horology as a whole, in the manufacture of, say, the common "18-size full plate, keyless, four-pair jewels" watch movement is, by count of the twenty-five foremen of the different departments in which it is made up, about 3,746.

The American system of manufacturing by interchangeable parts means much more than making a part under the roof of a factory, buying other parts in the market, and obtaining other parts by the piece from workpeople who live in their own cottages, for which they are paid at piecework rates. It means the establishment of working facilities for the entire manufacture. That everything is made on the premises, not according to the plans or ideas or methods of work of individual workmen, but under the direct supervision of a company's foreman, according to gauges the company furnish, under conditions of time, cleanliness and care which the company prescribe. The operative himself is a machine. There is as little as possible variation in the drill of a great factory.

The results of this are shown in a promptness of delivery, in a uniformity of products, utterly unattainable under the prevalent methods of what is called the factory system outside of the United States where the work of operatives is collected from their local habitations to be made up into timepieces at a central putting together establishment.

A little later in the evening we shall see, from the photographs, the various departments of a great watch factory, and I pass now to other considerations of interest to you. It is already apparent to you that the master-mind of such a company is to be chosen for his general executive qualities, and his ability to conduct a manufacturing business, rather than his technical knowledge of watches. In 1884 there were 2,500 operatives in the factory we have quoted. Only a few, and those in the designing and putting together departments, were professional horologists. There are machinists, and draughtsmen, and die sinkers, and steel workers and gilders, and enamelers and photographers, and men of many trades, each working in their own departments. Their tools are found by the company, they work on the company's premises, they report at a given hour, have certain intervals for rest, and go home at another hour. They are exposed to the strictest supervision during the progress of their work, and they have every attention given to their personal comfort. Plenty of light, plenty of fresh air, and a comfortable warmth pervades every part of the enormous buildings, to which the 2,500 (in 1884) operatives repair every morning for their duties. Taken all in all, I believe it is the most intelligent body of operatives in any industry known to me. Making up, as they do, the community in a very cultivated part of New England, they have every opportunity in the way of libraries, literary clubs, church life, and social advantages. One is very much impressed with the intelligence, taste, and general attractiveness of the operatives generally, and the women in particular, as they sit at their benches and machines. The delicacy of their work and the *esprit de corps* of the entire body are the two conducing factors to this result. In giving employment, a good character is insisted on. It is rather a remarkable fact that almost the entire number are native born American, with very few, almost none, of the Irish, who are most numerous in other systems of manufacturing. The morality of the young women is on a par with their general intelligence. It is very rarely indeed, that one is found against whom the breath of scandal is heard. Marriage amongst the operators is frequent, and the romance of life is not ignored.

Homes are made as well as watches. There has grown up in the factory the spirit of interest in its social associations, in its products, and an interest in its reputation, which can only come of a large, industrious, intelligent, well-fed and well-paid community. The company finds it to their interest to maintain these feelings among the operatives. Parks are laid out; cottages, which are both tasteful and healthful, are built; good drainage and ventilation are secured; trees, flowers, lawns are used to beautify the homes, streets, and grounds about the factory buildings. A large hotel has been built for the exclusive use of the young women who are not married, and who wish to live in it. I had the pleasure of dining at this hotel, in the dining-room at the tables of which about 120 young women were seated. The meal was nicely served and consisted of roast beef, roast mutton, potatoes, corn (maize), turnips, with apple pie and coffee and cheese for dessert. The charge which the company makes for board alone is $2.25 per week, or for board and lodging, $3 per week. Breakfast is served at half-past six in the morning. Now, when we consider the average pay of the women on the rolls is $7.98 per week, it can be understood that they have a margin for dress and other things which makes their life quite attractive to them. With their general intelligence there is borne the natural desire to become skillful and expeditious in their work. They become specialists with the new machines put into their hands. They know machinery to be their friend, for anything which will make two watches where one was made before, will halve the cost of the watch, but will not halve their wages, and will make their services still more valuable to the company. They are not afflicted with trade notions or customs; they are thoroughly malleable in their methods of work, and quick to do any new thing required of them. They are inventive and suggestive.

Under the administration of such great establishments there have grown up completely new methods of work. The claim that the machinery, processes, methods of organization of a great American watch or clock company are not original, is as ridiculous as to say that the Falls of Niagara are not original. The one is as much a product of the inventive genius, the disregard of trade laws and customs, the intelligence and necessity of production of an Anglo-Saxon country, separated by 3,000 miles of water from any other Anglo-Saxon country, as the Falls of Niagara are the product of the waters of the lake basin, and the topography of its outlet. The machines of these places are unique. They must be so, they are made on the premises to suit the individual needs of the establishment. For some of the important processes there are as many kinds of machines as there are distinct factories in the United States.

So far we have been referring to the middle class of watches, made in lots of many thousand at a time, and designed for general use. But it is evident that it needs only the addition of the adjuster's skill to produce on the same system, timepieces of a very high precision of running. So the leading American watch companies have put forward the claim that it is only necessary for them to add an adjusting department to their factory organization, and they will produce timepieces which will be much less in price, and equal in performance to the timepieces made by the very best foreign producers on the old systems. So such adjusting departments have been added to several of the leading factories. In these departments a watch is rated and adjusted to position and temperature for periods ranging from a week to six months, depending on the price at which the movement is to be sold. The watches which are meant for the very finest time-keeping are adjusted at two extreme temperatures, and in the fine positions of dial up, dial down, pendant up, pendant right, and pendant left. The adjustment of a watch is somewhat analogous to the adjustment of sights on a rifle. All the parts of an admirable weapon may be there, and the sights may be approximately right; but it takes actual trial and the slight adjustment of the sights to make any given rifle do its most accurate work.

As far as the success attained by the American method in watches of the very highest class is concerned, I can do no better than to quote the results of the trials of timepieces at the observatory in Yale College, made under the direction of the Corporation of the College for the

years 1881 and 1884 inclusive. Their trials are conducted at the Observatory of Yale College, and involve the daily comparison of watches in the following positions and temperatures, for a total period varying from six weeks for watches of the highest class, down to twelve days for watches of simpler adjustment, according to the following schedule:

Test No.	Position.	Approximate Temperature.	Number of days trial for Class Certificates.			
			I.	II.	III.	IV.
1	Dial vertical, pendant up	60° to 70° F.	7	8	8	12
2	Dial vertical, pendant right	60° to 70° F.	2	2	—	—
3	Dial vertical, pendant left	60° to 70° F.	2	2	—	—
4	Dial horizontal, dial down	60° to 70° F.	2	—	—	—
5	Dial horizontal, dial up	60° to 70° F.	10	8	8	or 12
6	Dial horizontal, dial up	34° to 38°	1	1	1	—
7	Dial horizontal, dial up	95° to 100°	1	1	1	—
8	Dial horizontal, dial up	60° to 70°	10	—	—	—
9	Dial vertical, pendant up	60° to 70°	7	—	—	—
	Total number of days	——	42	22	18	12

The conditions excluding watches from receiving certificates of any class were as follows:

1. When the variation of rate with the dial vertical and pendant up in classes I., II., III, and in the positions indicated in class IV., exceeds 2s.0 from one day to the following day.

2. When the variation of rate between the positions of "dial up" and "dial vertical" exceeds 10s.0

3. When the variation for 1° F. exceeds 0s.3 between the ordinary temperature and the oven.

4. When the rate is greater than 10s.0 per day in any position.

The number of watches entered for the period under consideration was as follows:

1880–81	219
1881–82	53
1882–83	41
1883–84	87

and the analysis of the results of watches of the highest class for the four years, is as follows:

	1880–81.	1881–82.	1882–83.	1883–84.
Per-centage of watch movements receiving certificates of any kind, excluding watches entered for rate records	45	38	28	77
Number receiving Class I. certificates	22	12	8	42
Average mark of Class I. certificates	68	68.6	76.4	74.4
Average mark of the first five watches receiving Class I. certificates	79.4	77.0	80.4	85.8
Highest mark received during the year	83	82	85	90 4
Makers' name of watches receiving the mark during the year	American Watch Co. Waltham.	Barraud & Lunds, London.	Vacheron & Constantin, Geneva	American Watch Co., Waltham.

In reference to these "marks" I would like to explain that they are affixed to certificates on a scale of 100, of which 40 are awarded for a perfect position adjust-

ment, 40 for the capacity of a watch to run at a uniform rate from day to day in any given position, and 20 for a capacity to run without a change owing to ordinary changes of temperature. The table shows, therefore, that in two of the four years' trials one of the American companies entered watches which were the very best in performance of all the watches entered, and that the watch showing the highest record of any entered, was one of American manufacture. I quote these results as conclusive evidence that by the American system of machine watchmaking, watches may be, and are produced, having all the fine running qualities of the best watches made under the ordinary system at a much greater cost.

DISCUSSION.

Mr. Whipple said, that having had an opportunity of testing some of the watches made at the Waltham factory, he could bear out the statement as to their excellent going qualities. The scheme for giving marks indicating the value of watches as timekeepers was the same at Kew as at Yale, and he had to thank Dr. Waldo for the great assistance which he had rendered in the elaboration of the present system. He believed the system was very closely analogous to that which had been employed for many years at Geneva, with one exception, viz., that at Geneva and Neuchâtel monetary prizes were awarded for watches which obtained the highest number of marks. In England, watchmakers had become alive to the necessity of improving the manufacture, and they had come to the conclusion that there was some little good in rating watches and having them certified, though at the outset they ridiculed the idea. He knew of an instance where a customer had been told that a Kew certificate was worth nothing at all, as it did not mean the watch would go well; but upon the customer pressing for a certified watch, he found that the certificate added fifty per cent. to the value. Another difference between the Yale and Kew rating in the adjudication of marks, was that at Geneva a greater proportion of marks were given to accurate compensation. Dr. Waldo and several horologists had given it as their opinion that the attainment of practically correct temperature compensation was comparatively easier than the attainment of correct rating in position, and his experience tended in great measure to confirm this. Many more watches failed to obtain certificates on account of their not going correctly in varying positions than on account of variation in temperature; and the art and mystery of compensating for temperature appeared to be very well mastered by the makers who sent their watches to Kew to be certified. With regard to making watches by machinery, he might mention that this was now done at Coventry, though whether successful from a financial point of view he was unable to say. He had been told by one of the managers that in one wing of the factory watches were being made by hand, and in another by machinery, with a view to a comparison being made of the relative cost of production by hand and machinery. Many parts of a watch must of necessity be better made by machinery than by hand; but other parts, such as screws, could be turned out at less cost by hand by the operatives, as the number required was not sufficient to pay for machinery. As the escapement wheel required to be made to the greatest nicety, it could in all probability be made much better and more cheaply by machinery than by hand. Considering the large number of persons of the lower classes who were out of employment, and whose services could be obtained at a cheap rate, he doubted whether it would be a profitable speculation to erect machinery in England for turning out screws.

Mr. H. Ganney, speaking as a practical watchmaker who had been at the Waltham factory eighteen years ago, and as one who had written and lectured on the subject, said he hoped the outcome of the reading of this paper would be something practical for the art in general, and also to watchmakers in particular. No doubt England had lost a good trade, and for this they had to thank the British Parliament, which, in 1843, refused to grant a charter to a company for the manufacture of watches by machinery. Ingold, to whom reference had been made in the paper, after passing his time between England, America and Switzerland, died almost unknown, but one of his machines was still to be seen at the

factory of Messrs. Gillet and Bland, at Croydon. Watches were now made in London by Mr. Nicholl, by means of machinery, and also by Mr. Gee, who employed over 200 men, and it would be many years before the Americans would be able to turn out watches to equal these. There were two ways of looking at a watch, first, as a work of art, and next, as a piece of commerce, and his own opinion was that they had not yet seen all that could be done in this direction. However great the success of the American companies, they had not yet surpassed the watchmakers horologically. Neither did they put such beautiful work into their watches as would be found in an English watch. He could not agree with Mr. Whipple that it would not pay to make screws by machinery on a large scale if you only wanted a few at a time, because he knew as a matter of fact that it did pay. On behalf of the Clerkenwell watchmakers, he acknowledged that they were under great obligation to the Waltham Company for what they had taught them, and thought that if they were sensible they would take the lesson to heart and endeavor to beat the company.

Mr. Bedford begged to call attention to the fact that, in speaking of the factory at Waltham, Mr. Ganney was speaking of what he knew eighteen years ago. Of course, in the present age, eighteen years was almost like a century, as things changed so rapidly. The factory and machinery which was there eighteen years ago was now all gone.

Mr. Ganney said that Mr. Dennison, of the Waltham factory, was now in Birmingham, and had been there eighteen years, and he established a factory in England sixteen years ago.

Mr. Robert Bragge (Secretary of the English Watch Company) was extremely gratified to find that thanks were given to those to whom they were due, and that at last the labors of Mr. Dennison had been acknowledged. The reason given by Dr. Waldo for the enormous strides that machine watchmaking was making in America was the true one, that the people there found themselves face to face with necessity, besides being unencumbered by tradition. It was a new field, and they entered upon it and went ahead; but in England they were not so happily placed. If there was one disadvantage under which workmen were placed, it was that of holding too much to tradition, from which it was extremely difficult to emancipate them. They made watches as they did for the same reason that the Dutch built their boats, viz., because their grandfathers had done so before them. In Birmingham, there was a factory which turned out a considerable number of watches by machinery; and it was only by the application of machinery, and following on the lines that had been indicated that evening, that watchmaking could hold its own in England. The company with which he was connected had sent a good many workpeople to America, but up to the present, they had not had the chance of sending any watches, owing to the heavy import duty; but he had no doubt that by-and-by they would be able to do even this. In conclusion, he begged to offer his tribute of admiration for the extremely fair and candid spirit displayed in this paper, and hoped that it would spur Englishmen up to a spirit of emulation, and lead them to achieve similar feats to those which had been accomplished in America.

Mr. G. William Frodsham (Parkinson and Frodsham) thought that the thanks of all watchmakers were due to Dr. Waldo for having given them such a clear insight into the industry of watchmaking by machinery in America; but the system was not quite unknown to English manufacturers. He had just made the model of a machine for making ordinary watches, which he intended to take out a patent for in America as well as in England. When American watches were first introduced into England, he made the remark that he was very glad the Americans were undertaking to supply the millions, because it would give English makers time to devote to the manufacture for the hundreds, that was to say, the aristocratic watches, and he saw no reason now to depart from this view. Reference had been made to an English company which did not succeed in consequence of the opposition of Parliament; but this was, no doubt, owing to the fact that in those days there was a strong prejudice, more particularly on the part of workmen, against the introduction of the machines designed by Mr.

Ingold, so much so that at one time this gentleman went in fear of his life, and no doubt the views entertained by the watchmakers and workmen influenced members of Parliament, who, of course, had no technical views upon the subject.

Mr. Liggins said that for a large numbers of years the English public had not been able to get a well-made, good English watch, except at a high price, and therefore, it was time that English manufacturers began to bestir themselves. No doubt many would recollect that some forty or fifty years ago a public company was attempted to be got up for the manufacture of watches by machinery by Mr. Barwise, of St. Martin's-lane, but it was found impossible to work the company, owing to its being opposed tooth and nail by watchmakers and working men. He had not seen a watch factory in the United States, though he had in other countries, and he could bear testimony to the correctness of the statements made with regard to them. He considered it a dire misfortune for the English public and the trade of the country that the export in watches was falling off. As a rule he did not think this applied to high class watches, for it was generally found that when a colonist visited this country he took back with him a first-class English watch; it was the trade in cheap watches that was being lost. For rough usage a machine-made watch had many advantages over a hand-made watch, price and quality being considered.

Mr. Glasgow thought that a good many of the remarks which had been made that evening had been of a personal nature, and not in elucidation of the paper. Dr. Waldo had read a paper which had been highly appreciated, in which he had not introduced any controversial subject. Having done the Horological Institute the honor of calling upon them the previous evening, he said, "This is what we want in America," which he (Mr. Glasgow) took as a compliment to England. It was supposed by some people that watches were all made by hand, but this was a fallacy, there not having been a watch made entirely by hand for centuries. The question to be considered was, how far could machinery be adopted with advantage to the great watchmaking community of the country. If there had been as many watchmakers in the United States as there were persons in that room, watchmaking by machinery would not have existed; it was the necessity that caused it. On behalf of the watchmakers in general, he expressed his gratitude to Dr. Waldo for his interesting paper. There was one remark in the paper with which he could not quite agree, viz., that the workpeople were "machines." Was it by machines that watchmaking had been brought to its present state of perfection? Were the great watchmakers of France "machines?" If so, watchmaking would not be what it was at present. They must not conclude that because the cheap grade of watch was made by machinery, that machinery would do everything. He believed that watchmakers were very much pleased when the system of testing watches at Kew was first established; but there was one practice which they did not appreciate, and that was this, that when a man made a first class watch, and received a certificate for it, the certificate could be changed to any other person on the payment of 1s. This practice, he thought, was not conducive to the interests of watchmaking.

Dr. Waldo did not believe there was anyone who had been a student of theoretical horology who had a higher appreciation than himself of the genius of the distinguished men to be found in the list of British horologists. There was no finer record to be found than the names of Harrison, Earnshaw, Reid, Dent, Frodsham and others, but he had not intended to present the scientific side of horology; it was merely the great art of making timepieces for a gradually growing civilization over the large world that he came to speak about. That was the question to be considered, and not the question of absolute science of precision in timepieces. The question which the Americans were facing was that of making watches which should keep time at the lowest possible price. He did not know that there were any questions which required an answer at his hands, and he would conclude by thanking the meeting for the kind way in which it had received him.

The Chairman, in proposing a vote of thanks to the reader of the paper, re-

ferred to the extreme interest of the subject, and the admirable way in which that interest along many lines had been developed. The question of the watch was really one of national importance, and there were even interesting side issues which almost rose to the height of political economy, in connection with the manufacture of such an article under new conditions, as opposed to its manufacture under old conditions. It was clear that the sense of the meeting was that England did lose once upon a time a very fine opportunity of expanding the watch trade, when the indications of science were pointed out a good many years ago. He thought that was not only true of the watch trade and of England, but it practically must be true of all trades and in all countries. His interest in the paper had come from the very great interest he took in the more scientific side of the subject, of absolutely accurate time-keeping, and his knowledge of the very great interest which Dr. Waldo had given to this matter. They must not forget in the large political questions the question which always ought to be before anyone interested in a great industry, viz., what can science do to improve it. That would yield to no question in importance. There was still room for the introduction of science into watchmaking in this country, whether the establishment of large companies was insisted upon or not. One of the most interesting parts of the paper to his mind, was the statement of the different scientific processes carried on under one roof; and it was not only the watch, but science generally which would gain in all these localities. With regard to the watchmaking industry at Coventry, he might say that many years ago he was asked to distribute the prizes at the Science Schools in that town, and he then had to tell the people frankly that if there was a little more science teaching in Coventry, it would be better for the watchmaking carried on there. That remark did not apply alone to Coventry; it was applicable to many other places. It was in this direction that the extreme value of meetings like the present would be found, for not only did it enable them to see what had to be done, but it furnished the key of the citadel which had to be attacked in the future.

THE SELF-INDUCTION OF AN ELECTRIC CURRENT IN RELATION TO THE NATURE AND FORM OF ITS CONDUCTOR.

By Professor D. E. HUGHES, F.R.S., President.

Inaugural Address delivered before the Society of Telegraph Engineers and Electricians.

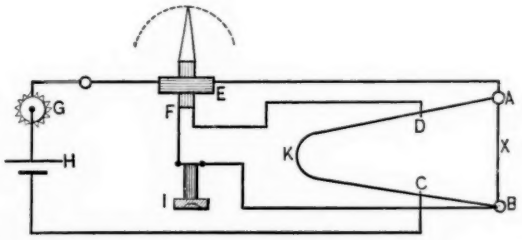
Induced or secondary currents in a near but independent circuit were discovered by Faraday in 1831; and the phenomenon of the self-induction of an electric current in its own wire was observed by Henry in 1832, and traced to its cause in 1834 by Faraday, who proved that on sending a current through a wire a momentary induced current in the opposite direction is evoked in its own wire; also that, on the cessation of the primary current, a second induced or "extra current" is excited in the direction of the primary. The effect is greatly augmented when the wire forms a coil, as we then have in addition the reaction of superposed currents; but the effect exists to a great extent even when the wire forms but a single loop, or a straight wire with the the earth forming the returning portion of the loop, as in all telegraph lines. It has been generally supposed that the nature or the molecular condition of the metal through which the primary current passed exerted no influence upon the extra currents except that due to its resistance. I have previously pointed out that for induced currents "the rapidity of discharge has no direct relation with the electrical conductivity of the metal, for copper is much slower than zinc, and they are both superior to iron." This led me to make a study of these extra-currents, for which purpose I constructed a special induction bridge, in order to measure both the primary and its extra cur-

rents separately at the instant of action.

INDUCTION BRIDGE.

This instrument is a combination of a portion of my "induction balance," with a "Wheatstone bridge." The resistance of the wire is measured and balanced by the bridge; the induced or extra currents are measured and reduced to zero by an equal opposed induced current from the induction balance.

The diagram herewith shows the electrical communications. The bridge consists of a single German silver wire (0.25 mm. diameter, 1 meter in length, of 4 ohms resistance) running from A to K, returning to B. The wire is stretched and sustained upon two wooden arms articulated at K, by means of which the terminals A B can be more or less separated as desired. The wire to be tested, X, is joined at A and B, thus completing the closed circuit of the bridge. The



external communications are shown, A being connected to the primary coil of the sonometer E, and through it to the spring of the interruptor or rheotome G, the interrupting wheel being connected to the battery H, and thence to the bridge at C. The wire from B passes through the telephone I to the secondary coil F, returning to D. Great care has to be taken in the construction of the bridge, so that it shall be as free as possible from induced or extra currents, and for this reason we cannot employ or introduce resistance coils. The resistance of the wire X is balanced by sliding the communications D and C. It is evident that if all the arms of this bridge are equal in resistance and inductive capacity, there will be silence on the telephone; but if A B be slightly stronger or weaker in inductive capacity, then we may be able to balance its resistance, but not its induction, as we shall then have a slight or a loud continuous sound due to the differential extra currents in the arm A B. These are compensated by the introduction in the circuit of the telephone of an equivalent but opposed induced current from the secondary coil of the sonometer F, the degree of angle through which this coil has turned to produce silence being the degree of force of the extra current. The induction sonometer consists of two coils only, one of which is smaller and turns freely in the center of the outside coil. The exterior coil being stationary, the center coil turns upon an axle by means of a long (20 cm.) arm, or pointer, the point of which moves over a graduated arc or circle. Whenever the axis of the interior coil is perpendicular to the exterior coil no induction takes place, and we have a perfect zero; by turning the interior coil through any degree we have a current proportional to this angle, and in the direction in which it is turned. The valve of the induction current for each sonometric degree was $\frac{1}{2500}$ of the primary current which passed through the wire under observation, the latter being variable at will from .001 to .250 ampère. There is also a reversing key (not shown in the diagram), in order to place the interruptor on the telephone circuit and close the battery current from H to A; the conditions then being the usual method of testing, except using the telephone in place of a galvanometer—a well-known method. The telephone, being exceedingly sensitive and rapid, is most suitable, whilst a galvanometer would be too slow, and its use, in fact, impossible for the researches I have been making. Numerous details have been necessarily omitted in this rough sketch of the instrument. Suffice it to say that it is perfectly adapted for the object

sought—viz., the investigation and measurement of the self-induction which takes place in all wires.

By all previous methods the measurement of the resistance of a wire is taken when the current has been already some time in action, or, to use an expression of M. Gaugain, when the electricity has arrived at its "stable period." In telegraphy, electric lighting, and all applications using rapid electrical changes, another period has to be considered, viz. that during the rise and fall of the current; this he named "the variable period," and it is in this period that all the phenomena of induction take place. To observe the stable period, the current is continuously passed through the bridge (and consequently through the wire under observation), and the interruptor being placed in the telephone circuit allows us to find the exact resistance of the wire, free from all induction or change in the wire itself. To observe the variable period, the interruptor or rheotome (making at will from 10 to 100 contacts per second) is placed on the battery circuit, the telephone being joined as shown in the diagram. By means of a switch or reversing key these changes are made as rapidly and often as desired. If there were no static or self-induction, no loss of time, or change of resistance, then the result from these two periods would be equal; but this is never the case, for we find that when the resistance is balanced to a perfect zero for the stable period, loud sounds are given out in the variable period, requiring a fresh adjustment or balancing of the resistance of the wire, as well as a compensating opposing induction current from the sonometer to balance the self-induction. If we balance the resistance or the extra currents alone there is no possible zero, but when both are compensated we find at once a perfect zero for the resistance of the wire, and for its extra currents.

INDUCTIVE CAPACITY OF METALS.

The results of the following experiments prove that the force and duration of the extra currents depend upon the kind of a metal employed as a conductor, its molecular condition, and the form given to the conductor, independent of its resistance or the electro-motive force of the primary current. The increase of force by increased length is proportional to the length of wire less its additional resistance, but with wires of the same length increased cross section or diminished resistance does not produce a corresponding increase in the electro-motive force of the extra currents. The time of charge and discharge of the wire is independent of the electro-motive force of the extra currents; for, if we compare currents of equal electro motive force obtained from copper and iron, it will be found that the duration of these currents in wires of 1 mm. diameter, will be seven times slower in iron than in copper, and a still greater difference will be found in larger wires. The longest or slowest charge and discharge take place in the purest soft iron, and have a constant ratio of increase with increased diameter of the wire; my experiments giving for wires of double the previous section, or for wires of four times less resistance, a mean increase of three times its previous duration. The electro-motive force of the extra current in different metals will be seen in the following table, and in order that the values obtained from the sonometer may be clearly understood I have reduced the results to comparative values. The table of values were obtained on wires of similar length, having been repeated on a similar series of lengths ranging from 10 cm. to 5 meters. The instrument is sufficiently sensitive for pieces only 10 cm. in length, and the results from the short lengths were as pronounced and accurate as those for greater lengths. I may add that the instrument shows no effects or traces of static charge for the lengths mentioned.

TABLE I.—*Wires 1 mm. in Diameter, 30 cm. in length.*

Soft Swedish iron	100
Soft puddled iron	78
Swedish iron, not softened	55
Soft cast steel	41
Nickel*	34
Hardened cast steel	28
Cobalt*	24
Copper	20
Brass	13
Zinc*	12
Lead	10
German silver	7
Mercury*	2
Carbon*	1

* Being unable to procure wires of these metals, they were tested in the form of strips, and compared with similar strips of copper. Mercury was in a glass tube 2 mm. in diameter; carbon tested in the form of electric light carbon from 3 mm. to 10 mm.

The above table is only true for wires of 1 mm. diameter, as the effect depends upon the size of the wire in relation to the nature of the metal. In soft Swedish iron a diminution in the electro-motive force of the extra currents takes place with each increase in its section, and this has been partially foreseen by Maxwell, who said: "The electro-motive force arising from the induction of the current on itself is different in different parts of the section of the wire, being in general a function of the distance from the axis of the wire as well as time." From this I expected that the increase of electro-motive force by an increased section would not increase directly as its sectional increase; but I was not prepared to find, as my experiments prove, that after a certain maximum diameter of wire has been reached a marked decrease in electro-motive force takes place with each further sectional increase, and that this maximum is variable with each metal.

When these values are graphically represented, the diagram shows a rapid rise of force in soft iron from an extremely fine wire of 0.10 mm. section to a maximum at 1 mm., from which point there is a slow but continued decrease of force with each increase in the size of the wire, until at the comparatively great diameter of wire of 10 mm. the force is but a fraction more than in the extremely fine wire. Hard Swedish iron has a less initial force in the fine wire, and does not arrive at its maximum until the wire has 3 mm. diameter, being then nearly of the same force as soft iron of the same diameter; the fall from this point is somewhat similar, but less than soft iron, until 8 and 10 mm. soft and hard iron have absolutely the same values. A curious change of values at different diameters is seen in copper and brass. Copper, having nearly double the initial force in fine wires, arrives at its maximum at 4 mm.; but brass creeps slowly up, passing copper at 5 mm., arriving at its maximum at 6 mm., and finally in the large section 10 mm., it has more force than copper, their positions being completely reversed. I have been unable to obtain wires of different diameters of other metals; but zinc rods of 10 mm. gave a still higher rate than brass, whilst in small diameters its force was less. For non-magnetic metals it is probable that the greater the specific resistance of the metal the greater will be the diameter of the wire before the fall commences. Carbon is remarkably free from self-induction, and although there is a rise of force in rods of 3 mm. to 10 mm., it is so small as to be hardly measurable. German silver rises with comparative rapidity, indicating that with wires of 20 mm. its force would equal that of copper. Carbon, therefore, seems peculiarly adapted as a resistance when used in the variable period of electric currents.

INFLUENCE OF PARALLEL CURRENTS.

The instrument being well adapted for showing the slightest change in the self-induction by the reaction of one portion of the current upon the other when in the same direction, as in a coil, or in the opposite direction, as in a parallel return wire, I made a series of experiments in order to observe the influence of different metallic conductors in this respect. Two silk-covered iron and copper wires of similar diameter and length (1 mm. diameter, 2 meters in length) were each formed in a single loop of 66 cm. diameter. The extra-currents from iron were, as usual, six times stronger than those from a similar loop of copper. On closing the loop by bringing the opposite sides in close proximity, and thus making a parallel return wire (the current ascending on one side and descending on the other), I found that the reaction of currents in opposite direction was very different with different metals, the results depending more upon the nature of the metal than upon the proximity of the wires. There was a reduction of the previous force of the extra-currents of iron, when forming a parallel return wire, of 15 per cent., whilst the reduction in copper was 80 per cent. Thus the currents in copper are far more influenced by an external wire than those in iron; consequently, a telephone line having its return wire in close proximity should invariably be of copper, as not only is its specific inductive capacity less, but this is again reduced by the return wire, so that its self-induction is far below that of iron. In order to observe the influence of currents in the same direction, the same wires were formed into a close coil of twelve turns of 2 cm. diameter; and from the known effects of parallel currents in

the same direction we should expect a greatly increased effect. It was so in the case of copper, but iron was far less under the influence of an external parallel current; the strength of current in iron when formed into a coil being 57 per cent. greater than that of a single wide loop, whilst in copper the increase was 404 per cent., or seven times the increase of iron; and although iron when in a single wide loop had six times the force of copper, the comparative strength was reversed when the wires were wound as a coil, the extra-currents from the copper coil then having 14 per cent. than that from iron, and this difference could be rendered more evident by employing longer wires. Thus copper, as regards extra-currents, is far more sensitive to the influence of external currents than iron, and the true self-induction from its own current can only be obtained by a straight wire, where the return wire is at such a distance that its influence is not appreciable.

REACTION OF CONTIGUOUS PORTIONS OF THE SAME CURRENT.

It is well known that currents in separate portions of the same wire (as in a coil) react upon each other, and I felt convinced from the preceding experiment that self-induction is entirely due to similar electro-magnetic reactions between contiguous portions of the current in its own wire. Let us assume that an electric current consists of a bundle or an almost infinite number of parallel currents, the limit being a single line of consecutive molecules; then each line of current should, by its electro-magnetic action, react on each of the others similarly to wires conveying separate portions of the current, and the self-induction should be at its maximum when the lines are in the closest possible proximity, as in a conductor of circular section, and far less when separated, as in one of ribbon form, where the outlying portions are separated by a comparatively great distance; there would still remain, in the latter case, the reactions from the near portions on each other, and these should again be reduced by cutting the ribbon into a number of thin, narrow strips, separated, except at their junction, to a sufficient distance to prevent any marked reaction. My experiments prove that this assumption is an experimental fact, for we can reduce the self-induction of a current upon itself to a mere fraction of its previous force by simply separating, as indicated, the contiguous portions of a current from each other, the results proving that a comparatively small separation, such as is obtained by employing ribbon conductors, in place of a wire of the same weight, reduces the self-induction 80 per cent. in iron and 35 per cent. in copper; and if we still divide the current by cutting the ribbon into several, say sixteen, strips (separating the strips at least 1 cm. from each other), then the combined but separated strips show a still greater reduction, being 94 per cent. in iron and 75 per cent. in copper. The following table shows the comparative reduction of self-induction by employing ribbons and parallel separated wires:

TABLE II.

Flat Strips compared with Round Wire 30 cm. in length.	Copper.	Iron.
Wire 1 mm. diameter.	20	100
Strips.		
0.25 mm. thick, 2 mm. wide....	15	35
Same, 5 mm. wide.....	13	20
Same, 10 " "	11	15
Same, 20 " "	10	14
Same, 40 " "	9	13
Same strip rolled up in the form of wire.....	17	15

Parallel Wires 30 cm. in length.	Copper.	Iron.
Wire 1 mm. diameter.	20	100
Single Wire.		
0.25 mm. diameter.....	16	48
Two similar wires.....	12	30
Four " "	9	18
Eight " "	8	10
Sixteen " "	7	6
Same, 16 wires bound close together.....	18	12

The resistance of a conductor, or even the nature of its metal, has less influence on its self-induction than the form given to that conductor, the 1 mm. wire in the above table having a less resist-

ance than the strip of 2 mm. wide, and a greater than any of the wider strips; but through all these variations we notice a gradual fall from the wire to the widest strip or ribbon, with a marked return to its previous force when the ribbon is rolled up in the form of a wire. The reduction is greater in iron than copper, but its increase when rolled up is less than copper, thus agreeing with the previous observations on the difference of iron and copper to external reactions. A still greater reduction takes place when we separate a current by using parallel wires separated 2 cm. from each other, as shown in the table. We then have a similar reduction to that produced by cutting the strips into several separate conductors; and we again remark that when the wires are brought close together (forming a stranded wire) copper rises in a far greater proportion than iron, the sixteen fine iron wires twisted together as a stranded wire having 88 per cent. less induction than a solid wire of similar weight; a remarkable fact being that whilst a solid iron wire has an inductive capacity 80 per cent. greater than a solid copper wire, this is completely reversed when each metal forms a stranded wire of the same weight as the solid, for iron then has 33 per cent. less self-induction than copper. It is not necessary to use extremely fine wires when we desire to reduce the inductive capacity of iron to that of copper, for I have formed stranded wire rope of sixteen strands of wire where each wire was 1 mm. in diameter, giving 75 per cent. less induction than a solid wire of the same resistance. I purchased an ordinary wire rope of 6 mm. diameter, having 6 strands of 6 wires, each 0.5 mm. diameter. This gave the best result yet obtained; for on comparing 3 metres of it with a similar length of solid iron wire of the same resistance, the 36-stranded wire had only 5 per cent. of the amount of induction shown by the solid wire. Steel, in the form of ribbon or stranded wires, shows a similar effect to that of iron; and it is a remarkable fact that, whilst the extra currents from a steel or iron wire 4 mm. in diameter are extremely slow, and impossible to balance without reducing the time of the sonometer current (by the introduction of an iron core), the ribbon or stranded wire requires no such compensation, for its feeble extra-current is exceedingly sharp, and can be balanced to a perfect zero, being actually quicker than that of a solid wire of copper of the same resistance, This fact I regard as one of great importance for telegraph lines and lightning conductors.

A curious effect takes place if we employ mixed conductors, such as a compound wire of copper and iron. A fine coating of copper reduces the induction in a solid iron wire in a marked degree. This I found to be due to the difference of electro-motive force of the extra-currents in the two metals, for, by employing a fine copper wire parallel with an iron wire, and in contact at the ends, the extra-current was reduced 60 per cent. The copper wire, having a lower electromotive force, probably acts as a shunt; but if the capacity of the iron has already been reduced, as in a sheet or stranded wires, then the addition of a single copper strand increases the force, as the electro-motive force of the extra-currents of copper is above that of stranded iron. There has been for many years a discussion as to the merits of the round form as compared with the tape or ribbon form for lightning conductors. Those in favor of the former based their conclusions on experiments which gave a negative or no apparent difference between the two forms of conductors. Those in favor of ribbon conductors, as Sir W. Snow Harris, Professor Guillemin, and many others, based their opinion upon marked differences found when using high charges of static electricity. The latter supposed that there was a difference between discharges of static electricity and voltaic currents of low tension, and that the advantage recognized by almost conclusive experiments was due in a great measure to conduction by surface. In the year 1864, Prof. Guillemin and myself, as members of the Commission de Perfectionment of the French Telegraph Administration, were charged with the mission of testing the comparative methods of the lightning protectors then used upon their lines.

Our method of experimenting consisted in joining an insulated conductor to a short, fine iron wire connected directly with the earth return wire. A Leyden jar battery charged by a Rhumkorf

coil was discharged through this conductor, burning or deflagrating the fine iron wire. This wire represented the telegraph instrument requiring protection, and by placing the lightning protector connected with the earth in advance of the fine iron wire we could observe the amount of protection afforded. This answered extremely well for feeble discharges, but with the full power of our battery the fine iron wire was invariably destroyed, even with the best lightning protectors, which are universally used to this day. Noticing that we could not give absolute protection to the fine wire by lightning protectors, we tried the effect of joining the conductor direct to a separate earth wire in advance of the fine wire, and with powerful discharges the wire beyond the protection was invariably burnt, notwithstanding that we connected the conductor direct to earth by a copper stranded wire of 1 cm. diameter. Professor Guillemin continued these experiments after my departure for Russia, and he found, by employing a thin sheet of copper as a conductor to earth in place of the copper stranded wire placed in advance of the fine iron wire, that the wire could be perfectly protected. The theory of this action was not understood at the time, and the experiment has not received the attention it deserved; but the mutual reactions of contiguous currents shown in this paper explain the phenomenon in the fullest degree, for we see that a sheet or ribbon conductor has far less self-induction than a wire or rod of the same material.

I am fully convinced from the results of my experiments that an enormous retardation or resistance is evident in all conductors at the first portion of the variable period, and that this is due to self-induction, the current thus arousing an antagonist in its own path sufficiently powerful, when the primary current has a high electro-motive force, to deflagrate or separate the wire into its constituent separate molecules, as shown by Dr. Warren de la Rue. It is also evident from my experiments—which are easily repeated, with invariable results—that a flat conductor has far less self-induction than a solid of circular section during the variable period; and even with a constant current, as in the stable period, this form of conductor, as first shown by Professor George Forbes, would, from its greater radiation, convey more current with less heating than a wire or rod of the same resistance. Lightning conductors are intended to convey a current of high intensity during an exceedingly short time, and should therefore be designed so as to convey this current with as little opposition from self-induction as possible; consequently, I regard a solid rod of iron as the worst possible form for a lightning conductor. The conductor, if of copper, should be of ribbon form, say 1 mm. by 10 cm. wide, or, if of iron, of numerous stranded wires or a wide ribbon of similar conductivity to that of the copper.

SELF-INDUCTION OF A TELEGRAPH LINE.

A telegraph line may be considered as a single loop; the earth taking the place of a return wire can only affect the self-induction by a diminution of its effects, as in the case of a parallel return wire. Mr. W. H. Preece has lately read a most valuable paper on "The Relative Merits of Iron and Copper Wire for Telegraph Lines,"* in which he shows, by comparative rates of speed with the same instrument, that on a copper and an iron line of 278 miles in length (between London and Newcastle), whose resistance and static capacity were rendered equal, there was an increase of speed in the copper line of 12.9 per cent. as compared with an iron wire.

I have not been able to test the relative speeds obtainable by telegraph instruments on wires of different material. The results in every case would depend very much on the apparatus employed, but I have considered the question from a point of view independent of the instruments. There is a remarkable difference in the resistance of a wire during the stable and the variable period, the measurements taken in the stable period giving no real or approximate idea of what its resistance really is during the rise of the current in the wire. A curious fact in relation to telegraphy is that all measurements are made during the period of a constant flow of current, whilst all instruments, particularly those requiring rapid changes in the current—work only during the rise and fall of the

* British Association, Aberdeen, September 1885.

current, as in the variable period. Telegraph engineers, however, have not made the mistake of assuming that there is no difference in the resistance of a wire in these two periods, as it is well known that electro-magnets and coils have a far higher resistance during the rise and fall of a current, and coils simply augment the effect of a straight wire of a given length. The present method of testing by Wheatstone bridge has been adopted because we had no practical means of measuring the resistance in the variable period; and I do not believe that this can be accomplished except by a similar method to that which I have used, in which the resistance and self-induction are separately measured and balanced, and by the use of an exceedingly rapid and sensitive instrument of observation, as the telephone, in place of the sluggish galvanometers, no matter of what construction. The speed of telegraph instruments is greatly influenced by the resistance of the wire. I said in 1883* that a great difference could be found in the resistance of an electrical conductor if measured during the variable instead of the stable period, and I have made numerous experiments with the view of ascertaining to what extent this difference would probably be felt on telegraph lines.

I have already mentioned that the time or the duration of the extra-currents increases rapidly with the section of the conductor; consequently comparisons can only be made between wires of similar section for speed, or wires of similar resistance for differences in their variable period. In measuring the resistance of a wire during the two periods, I have found it best to avoid the use of resistance coils, the simplest method being to measure or balance a given length of wire in one period, and then observing how much lengthening or shortening of the wire would produce a similar zero in the second period. Suppose that we commence by balancing the resistance during the variable period, and fix the sliding communications at the point at which we have obtained a perfect zero; we can now change to the stable period by means of the commutator; and we no longer find a zero, but extremely loud sounds, we gradually lengthen the wire under observation until we have again a perfect zero. The amount of wire added to its previous length shows the difference in resistance between a conductor in which there are rapid electrical changes and that wherein the flow of current is constant. Amongt numerous experiments I will cite a single example. I measured or balanced the resistance of an ordinary soft iron wire, 1 metre in length and 4 mm. diameter during the variable period, and found that it required in the stable period exactly 2 metres 58 cm. to balance the previous resistance. Similar tests on a sample of best charcoal iron wire, as used on our telegraph lines, gave still more remarkable results, showing 225 per cent. difference between the two periods; for 1 metre of this wire had, during the rise and fall of the current, precisely the same resistance as 3 metres 25 cm. in the stable period. This shows that an iron telegraph wire has with rapid currents more than three times the resistance during its actual work than that supposed to be its true resistance. It was difficult on short lengths to find any change whatever in the resistance of copper or stranded iron wires in the two periods; the time of discharge being excessively rapid, I could only estimate the resistance by the electro-motive force of the extra currents, or by forming the wires into coils (when a remarkably great difference is shown), and then estimating the proportional amount due to its own reaction; by this method I was enabled to detect 20 per cent. difference for a solid copper wire, and but 8 per cent. for the stranded rope of 36 iron wires. The difference in time of the duration of the extra currents between solid iron and copper and between solid iron and stranded iron is so great that we may consider a solid iron wire to belong, comparatively speaking, to the class of slow conductors, whilst copper and stranded iron would belong to the rapid.

I have shown a difference of resistance in the variable period between copper and iron of at least 200 per cent.—a difference which will be felt on instruments depending upon rapid changes, such as the telephone; and it is evident that the more rapid the contacts of a

* Discussion on the paper of W. H. Preece on Electrical Conductors, *Proc. Inst. Civil Engineers*, vol. lxxv., 1883.

telegraph instrument the greater will be the difference between copper and iron. There is consequently a great electrical advantage in those instruments which require only a single current for each letter, as the economy of electrical impulses allows them to work at a comparatively high speed; the duration of the extra-currents would be shorter than the length of their contacts, and consequently they would perceive very little, if any, difference between the two periods, or between iron and copper. If we use three or five currents for each letter, we must necessarily send them faster or closer together; and the difficulty increases in a rapid ratio with the speed of intermittent or reversed currents, until a point is reached (as I have shown in the case of best charcoal iron) where, whilst nominally working through 500 miles, we are practically working through an equivalent resistance of at least 1,500 miles, and this without taking into account the static charge, which would, in addition, from its comparatively extreme slowness of charge and discharge, cause the apparent resistance of the wire in the variable period to be much greater than I have mentioned. In Mr. Preece's experiments he finds a difference of speed of 12.9 per cent, between iron and copper, which is far less than the difference of resistance during the variable period which I have obtained; and this may be explained by assuming that the speed of the reversed currents which he employed was only near the border land of extra currents. I am convinced that if Mr. Preece could have increased the speed of the instruments he would have found a far greater difference between iron and copper; and if I regard the results of a solid iron wire alone, I should consider iron as unsuitable for telegraph instruments requiring extremely rapid currents. Copper would reign supreme it were not for the fact, which I have discovered, that stranded iron wires have even a greater rapidity of action than copper.

PHYSICAL CHANGES IN THE CONDUCTOR.

Self-induction not only depends on the nature and form of its conductor, but also on the physical state of the metal, as already shown in the case of soft and hard iron. I felt convinced that the higher force in iron was due to its magnetic capacity, and to prove this I tried the effect of heating the wire to a bright red heat, or 1000° cent. It is well known that iron loses its magnetic properties at bright red heat, and I found that its self-induction then fell to less than that of copper. This would have been conclusive had it not been for the fact that a different result takes place when the capacity of the iron for self-induction has already been reduced, as in the case of thin flat sheets of iron. In this case there is no disappearance or further decrease of induction except that due to the extra resistance caused by the increased temperature of the strip. Now, as the strip was highly magnetic when cold, and lost this property at red heat, there should have been some change in its self-induction if this were due to the magnetic nature of the iron alone. This requires further researches before a probable explanation can be given. Iron is peculiarly sensitive to all physical changes. Mechanical strain of all kinds hardens the wire, and its influence on its self-induction can at once be detected. An iron wire under a moderate longitudinal strain loses 40 per cent., and its capacity is then less than unstrained cast steel. Iron well annealed has much less resistance than the same iron when hard drawn, and soft iron is generally employed for telegraph lines; but during the variable period a curious reversal takes place, as then soft iron has a higher resistance than hard iron. This apparent anomaly is easily explained if we compare the far higher self-induction of soft iron. Work is done at the expense of electrical energy, and the apparent higher resistance is due to the greater electro-magnetic action in soft iron. An iron wire shows traces of remaining circular magnetism after the passage of a continuous current, reducing the following extra-currents 10 per cent. Magnetising the wire, or subjecting it to mechanical vibrations, when used separately, produce no apparent change in its inductive capacity, but a remarkable change takes place if either of these is used in conjunction with a constant current. Let us pass a constant current and heat the wire to red heat, allowing it to cool with the current on; or, in place of heat, magnetise the wiire,

or, in place of magnetism, give the wire mechanical vibrations—the result of either of these being a strong internal circular magnetism, due, I believe, to the loosening of the magnetic molecules, allowing them to rotate with greater freedom under the influence of heat, mechanical vibrations, or magnetism. A wire thus treated has no longer its previous self-induction, which has fallen 60 per cent.; and as the circular magnetism becomes fixed when the vibrations cease, this molecular structure remains a constant as long as we employ intermittent currents in the same direction, but the structure disappears the instant a revere current is sent; and this explains why we have more than double the amount of self-induction from reverse currents, as each reversal destroys any remaining magnetism due to the previous passage of the current.

If we compare the electro-motive force of self-induction on a given length of wire with the secondary currents generated in a second but independent circuit, we find that the self-induction is the most powerful, the secondary currents generated in a close independent copper wire being 20 per cent. less than its own wire. There is no difference between the self-induction of a current and the secondary currents; they are, in fact, as proved by Faraday, part of the same phenomenon. The self-induction is evidently due to the electro-magnetic reactions of the primary current, and as magnetism permeates space, the separation of the wires only serves to insulate the primary, but does not affect its magnetic influence; and, as I have shown in the reactions of contiguous portions of the same current, so the magnetic reactions perpendicular to the axis of the current continue through the wire to all surrounding wires; and if we call the currents in the independent wire secondary, they are still secondary whilst enclosed in the wire of the primary; and as the reaction will ever be the strongest in the axis of the current, so will these currents be necessarily stronger than those induced in independent wires. For this reason we should be able to obtain extra-currents of far higher electro-motive force than would be possible from a secondary wire of the same length. It was my intention, on the reading of this paper, to demonstrate by practical experiments some remarkable properties of extra-currents of high electro-motive force; but I find that the subject and apparatus employed require a longer description than the limits of this paper allow. I must also leave aside for the present my experiments upon coils of different forms with cores of different metals. These, as well as other results obtained, indicate that there is a large field of useful research in many directions, each, however, requiring special studies according to the object we may have in view. The record of numerous experiments, of which this paper is only an abstract, shows that the nature of the metal as well as its physical condition has an important influence upon the self-induction of an electric current, and by a study of the reactions produced by the contiguous portions of a current, and by application of the results, we may, as in the case of iron, transform an extremely slow conductor into one of the greatest rapidity. I therefore hope not only that these researches may be of interest from a scientific point of view, but that the results obtained may be of practical utility in some of the numerous applications of electricity.

THE LIMIT OF SPEED IN OCEAN TRAVEL.

By R. H. THURSTON.

From "The Forum."

The questions are often asked: What are the causes which limit the speed of ocean-going steamers? Where are we to look for more favorable conditions than those now existing? What may we expect in the immediate future, and what, ultimately, as to the limit of increase of speed in steam navigation? The questions are easily asked, but, like many such problems, they will be readily seen to admit of no definite answer. No one can forecast the future in this age of multiplying inventions, of growing capacity on the part of the engineers to cope with

nature and to force her to give aid in conquering her mightiest opposing forces, and of continually-occurring victories of science and art, the one aiding the other, over apparent impossibilities, in every department of human activity. But we may at least feel our way somewhat beyond the present limit of our advance, and may, by careful study of the problem, and consideration of the principles of science and methods of art known to us, get some idea of what is before us.

The speed at which a ship can be driven through the water depends upon many, but well-known, conditions, the laws governing which have been, for the purposes of the naval architect, very well determined. Given the size and form of any well-designed craft, it is easy to predict, with a fair degree of approximation to accuracy, what amount of power will be demanded to drive the vessel at any proposed speed. This being known, it is easy to ascertain the size and form of the engines and boilers required, and to calculate their weight, bulk, and fuel consumption. It would thus seem that no unknown elements enter into the problem, and that a precise answer might be easily given to the question. That is not the fact, however, and it will be presently seen that there are very important factors, the value of which, and sometimes the nature of which, are not, and cannot as yet, be exactly known. It is proposed in the following paragraphs to consider the elements of the problem, and to discover where these uncertainties lie, to what extent they obscure the subject which we have taken up for study, and, so far as is possible, to obtain some idea of the extent, as well as the character, of the limitations which they involve.

The resistance of a steamship or other vessel consists of two principal parts. The effort required to overcome the friction of the water on its "wetted surfaces" measures the one, and the force expended in producing the waves that are seen arising about every ship in motion constitutes the other of these two quantities. Of these factors, the first is by far the greater in all well-formed ships, and such alone can be considered here. For every ship of a proposed size and weight there is a certain form and proportion of hull which is known to be best for the intended speed, and hence there is no great difficulty in securing almost exactly the best possible form, and thus of eliminating avoidable "head-resistance," or "wave-making" resistance, as the smaller of the quantities is termed. The friction of hull may be calculated, also, very approximately, as it is found to be very nearly proportional to the area of wetted surface. It is thus smaller as the surface of the hull below the water line is smaller. But it is evident that the nearer the form of the ship approaches that of a hemisphere the less must be the resistance due to friction, and that between the latter shape and that elongated and graceful form which gives minimum head resistance there must be some intermediate form which will give the least total resistance. The form of minimum resistance for a given size of ship must usually be felt out by careful experimental work. The solution of the problem last stated is, then, one of the elements of the problem of larger extent, that of maximum speed on the ocean. This solution is in process of being effected, and may be considered as having been already obtained with fairly satisfactory accuracy. The Oregon, now famous both for her speed and for her sad fate, and even more satisfactorily, perhaps, the "America," represent very excellent illustrations of highly successful attempts at a solution.

The power demanded to propel any vessel at ordinary speed varies as the square of her length nearly, or as the area of the transverse major section, and as the cube of the speed. Thus, to double the speed of any vessel requires eight times the power demanded at the lower velocity. Two vessels being of equal speed and similar form, but the one of twice the length of the other, the second will require four times the power of the first. The second vessel, however, carries eight times as much weight, and the power per ton of vessel is one-half as much as would be demanded by the first, if driven at the higher speed. These principles are modified by the relation of form to speed and size, and the rate of variation of increasing resistance of a badly-formed ship is greater than above stated; while, on the contrary, the well-formed ship may, at very high speeds, meet with a resistance which increases at a lower rate than the stated

law indicates. For vessels loaded to a limit with machinery, the higher the speed demanded, the larger must be the ship.

The impelling power of the ocean-going steamship is supplied by engines that have now become well fixed in their general forms and proportions, although signs of another revolution are already plainly discernible. The standard form of marine engine for merchant ships is a machine having its steam cylinders set vertically. It is of the "compound" type, *i. e.*, so arranged that the steam taken from the boilers is worked expansively to lower pressure in one cylinder, is then "exhausted" into a second larger cylinder, in which it is further expanded, doing work, meantime, until it falls nearly to the pressure of the condenser, and is then exhausted into the condenser, where it is condensed and returned thence into the boiler to be again evaporated. The condenser is called a "surface condenser," because the condensation occurs on the interior surfaces of the apparatus, which are kept cool by the flow of water along the opposite side of the metal.

The boilers supplying the steam to the engines of ocean steamers are usually of the Scotch type, consisting of a drum-shaped vessel, containing the furnaces and flues, or tubes, in which the fires are kept burning, and through which the flame, smoke and gases pass to the smoke-stack, heating the water contained in the boiler as they move over these heating surfaces of sheet iron, which surfaces are, on their opposite sides, in contact with the water to be made into steam. The larger these boilers the more economical are they, but the less powerful for their weight. Increased economy is always obtained at a sacrifice of power. Increase of speed thus means decreased efficiency.

The steam furnished to the engines will be used with greater economy as its pressure is greater, because it is worked with greater expansion as the speed of the engine is greater, and as the wastes, some of which are more or less controllable, are more effectively provided against. There are two great sources of waste—the one the unavoidable waste which occurs in consequence of the fact that the steam must be exhausted from the engine at such a temperature, and in such physical condition as to carry away a considerable amount of heat, partly sensible, and partly unrecognizable to the senses, and hence called by James Watt and Dr. Black, who discovered it, "latent heat"; the other is that waste which is due to the circumstance that all parts of the engine are made of metal, and therefore have high conducting power, and thus, by a process of storage and waste which is very interesting to the engineer, but which cannot be here described, often cause the loss of as much heat as is usefully applied. The first method of waste, in good engines, will often lead to the loss of three-fourths of all the heat of the steam that is supplied to the engine. The enormous waste to which the steam-engine is thus subject is reduced by steam-jacketing—by the covering of the engine cylinder with a jacket in which steam from the boiler is kept, in order to sustain the temperature of the internal surface of the engine—by superheating, and by high speed of the engine. The direct means of securing economy are increasing the steam pressure, with corresponding increase of the range through which the steam is expanded, and the reduction of losses of power in the engine and its machinery of transmission, including the screw propeller. The extent to which these several means of rendering the engine more effective and economical and useful, largely determines to what extent gain of speed at sea can be secured. It is further evident that the lighter and stronger the engine and boilers can be made, the higher the speed of vessel attainable. It has been often proposed to replace steam by some other fluid; but it is well known to men of science that the gain to be anticipated is theoretically *nil*, and engineers familiar with the steam-engine are well aware that not only are there no practical advantages of importance to be gained, but that many decisive practical objections exist to every other known fluid yet discovered and used in a heat-engine, in competition with steam.

The present state of the art may now be perhaps understood, and the probabilities of important advancement during the next generation may possibly be gauged with some degree of satisfaction.

The steamer Oregon, of which the name is now as familiar as as household word, may be taken as representative of the condition of the art at the commencement of the year 1886. She was a vessel of about 7,500 tons measurement, of 12,000 horse-power, and could, in a smooth sea, make about 20 knots (24 miles) an hour. The trip across the Atlantic was made in less than six and a-half days. Her length was 500 feet, breadth of beam 54 feet, and depth of hold 38. The America, a less noted, but no less wonderful vessel, is of 6,500 tons burden, 9,000 horse-power, and of very nearly the same speed. The smaller ship would seem to be the better illustration of the highest success in this direction. The Servia is 530 feet long, 52 feet beam, 44½ feet depth, and of 8,500 tons burden. Her power is nearly equal to that of the Oregon, and her speed something less. A still later example of the best modern naval architecture is the Etruria, a ship of 520 feet length, 57 feet beam, 41 feet depth, and 8,000 tons measurement. Her speed is about the same as that of the Oregon, but she is a larger, steadier, and perhaps better ship. Ten such ships placed stem to stern, as will be seen, would form a line one mile long.

But the most extraordinary performances, from the point of view here taken, are those of the steam-launches and torpedo boats built in the United States and Great Britain within the few years covered by the construction of the ships just described. The Herreshoff yacht, Stilletto, made more than 25 miles an hour not long since, and "showed her heels" to the Mary Powell, the fastest river steamer, probably, in the world. A torpedo boat built for the British Navy has made 20.14 knots an hour, and another 21 knots, while still another is reported by its builder, Mr. Thorneycroft, to the British Institution of Civil Engineers as having made 22.01 knots (25½ miles) an hour. These little craft are but 80 to 100 feet long, and of but 30 or 40 tons weight, including hull, machinery, and all. Their performance has excited the wonder of engineers as being enormously beyond anything yet attained by the larger vessels, the difference in size being considered.

Without attempting to assign a limit to the progress of naval construction in the coming years, we may be permitted to ask what might be done with a ship of a size now regarded as perfectly practicable, giving it the lines now regarded as the best for its maximum speed, a hull of minimum resistance to the flow of the water past it, and driving it by engines equal in economy, power, lightness, and general efficiency to the best yet designed and applied, and availing ourselves of every known means of securing the best result in the attempt to attain the highest velocity possible by these familiar methods, while yet retaining the conditions demanded of the fast transatlantic steamer.

It was asserted by a distinguished man of science, forty years ago, that no steamship could be made to cross the Atlantic, because of the impossibility of carrying sufficient coal to supply the engines and boilers for the voyage. The prophecy was proved false almost as soon as it was uttered by the appearance in New York Harbor of the Great Britain, the pioneer of the Cunard Line, after a passage of 14 days and 9 hours, and of the little Sirius beside her. A more credible recent prediction was made by a well-known naval architect, Mr. Robert Duncan, in 1872, who stated that he anticipated that, before the end of the century, we should see crossing the Atlantic the ferry-boats of the ocean, 800 feet in length. The Great Eastern was 680 feet long, and the difference between that length and 800 feet is not now to be considered very great. Let us assume that such a ship may be constructed, the question arises, what would be her maximum possible speed?

A steamer 800 feet in length, 80 feet beam, and of 25 feet draught of water, would weigh, complete and in sailing trim, about 38,000 tons, if given what may be considered as the best form to-day known for maximum speed. The fast ships of to-day exert about one and a-half horse-power per ton to reach a speed of 20 sea miles an hour. With some little improvement, such as may be safely anticipated before the close of the century, this figure may be reduced somewhat, and a larger ship will have some advantage. Our later Leviathan may be expected to demand about 35,000 horse-power at 20 knots. We will, however, aspire to 40 knots (about 47 miles), or a

speed of nearly one statute mile per minute. At this enormous speed she would cross the Atlantic in about 80 hours, or less than three and a-half days. The power requised would be calculated to increase as the cube of the speed; but it is, in fact, found that the law often becomes more favorable at these higher speeds, while a speed of 40 knots ecomically corresponds, according to what are known as "Froude's laws," to about the speed of the torpedo-boats, which latter are found to have reached a velocity well beyond the point of change of the ordinary law of resistance. We may take the probable power demanded as not far from 250,000 horse-power.

The weight of the steam machinery of vessels of various classes varies greatly, the maximum being several hundred pounds per horse-power, and the minimum falling, in the faster torpedo-boats, to a little above 50 pounds, while the yacht Gitana gives a still lower figure, 43 pounds. Progress beyond the latter point must be exceedingly slow, if we may judge by present appearances. These figures are partly attained by the sacrifice of efficiency, and we may perhaps fairly consider 60 pounds as the minimum to be calculated upon for this generation. Our machinery for the new ship will thus weigh about 7,500 tons. The fuel consumed by the most economical of known engines is much less than by the large steam engines of the transatlantic "liners;" but we may take the lowest figure for to-day as a fairly probable figure for this case. This is $1\frac{1}{2}$ pounds of good fuel per horse-power and per hour, or a trifle less; and our ship will burn about 175 tons of coal an hour, 3,200 tons a day, and 10,500 tons for the voyage. The total weight of fuel and machinery will then be about 18,000 tons, leaving 20,000 tons for weight of ship and cargo. The hull of such a steamer, as now constructed, would weigh about one-third the total "displacement," or 12,000 tons. The introduction of steel and the improvements to be effected in construction will probably somewhat reduce this weight; but it is not likely, so far as can be seen to-day, that the reduction will be very great. Eight thousand tons and over are left for passengers, crew, stores, and such valuable freight as may be taken.

It might be questioned whether the propeller of such a steamer could take up and usefully apply such an enormous power; but the experiments already made on torpedo-boats by Mr. Thorneycroft seem, in the opinion of that authority, to settle that point. He calculates that a single screw, of less size than those by which this ship would be driven, would be capable of transmitting the power of engines, "indicating," as the engineer puts it, about 400,000 horse-power.

Our proposed ship may be driven by "twin" screws. It may be asked whether economy is not to be anticipated, and to a very great amount, by the adoption of higher steam pressures. On this question there is no settled opinion among engineers. It would seem, however, that the gain to be anticipated will be very slight, and that a limit will probably be reached soon. Pressures of 150 pounds and more are already adopted in some cases, and the introduction of the "safety" form of "water-tube" boiler will probably soon permit still higher tension; but the gain of economy from this change is now found to be very moderate, and but little is expected from it by the majority of experienced naval engineers, except in decrease of weight of boilers. The boiler problem is exceedingly important; the weight and volume of the steam generator is a great obstacle to further advance. Increase of piston-speed may help us more. The maximum reached at present is about 1,000 feet per minute; but steam will follow the piston at any speed up to more than one hundred times that velocity. There seems no reason to doubt that the adoption of familiar principles in balancing may permit much higher speeds to be attained. The gain to be expected from increased expansion of steam is apparently not likely to be rapid, or to become very great, in the immediate future. Decreased weight of parts by the more extensive use of steel, and perhaps by the introduction of new metals and alloys, may prove helpful; but nothing positive can be said of this as yet. We certainly are not yet in a position to expect much. The gain by improved forms of hull, and by expedients looking toward the reduction of the friction on its exterior, cannot be expected to be im-

portant. Thus the question of increasing the speed of ocean travel seems likely to resolve itself into one of practicable size of vessel, and this means simply a question of cost and financial return. If higher speeds will "pay," higher speeds will be reached by the construction of larger ships. The limit is likely to prove mainly a commercial one for generations, so far as we can now see. To-day the fastest ships do not pay expenses, and the limit is reached in this direction. When more passengers and more precious freight can be found to pay for faster ships, faster ships will be built. The skill and knowledge of the engineer and shipbuilder will keep pace with the demand, so limited, far beyond any point that we can to-day perceive.

The wonderful effect produced by the application of human ingenuity to the development of inventions looking to the subjection of the powers of nature to the purposes of man, is well illustrated by these results of the introduction of steam power for the propulsion of vessels. Some slight idea may possibly be gained of our advancement in this direction, actual and possible, during a single century, by considering what is meant by the application of 250,000 horse-power to the propulsion of the ship here schemed out. The engineer's horse-power is the equivalent of the work of the strongest known horses when working at their usual rate in the ordinary working day. But the average horse is much less powerful, and it is safe to say that one-horse power, in the steam engine, is equal on the average to at least one and a third times the power of a horse. Then, again, the horse cannot work up to his average full capacity longer than about eight hours a day, while the marine steam-engine works continuously, day after day, the whole twenty-four hours, without halt or slacking its pace. Thus the engine horse-power is the equivalent of the operation of four horses, where the work is carried on without interruption. The 250,000 horse-power of the ship of the next century must be taken as the equivalent of the work of 1,000,000 horses. One million horses would weight about 1,000,000,000 pounds, or nearly 500,000 tons —over ten times the capacity of our ship, and nearly seventy times the weight of its machinery. The food and bedding of 1,000,000 horses for a single day would weigh probably 50,000 tons, or more than double the weight the vessel can float. Were this great herd of horses to be formed into a "string-team," allowing ten feet for the lenfith of one horse, and for the "clearance" between each two in the line, its length would be nearly 2,000 miles.

The cost of running the ship above schemed out would be probably not less than $75,000 for each voyage across the ocean; and the passage money of 500 passengers, at $150 each, would be required to pay this. Each passenger would save about four days' time, and four days of annoyance incident to the present method of travel; and this must be the equivalent to him for the increased cost. The ship could make a profit on freight and mails.

It must not be expected that the methods and details of construction which must be learned and applied properly in such a vessel are to be acquired promptly or easily. The problem of proper construction of the engine, or of the propeller shaft, alone, is a serious one which for a time may fail of solution, and may defer the realization of this speed for many years. There are hundreds of problems that the engineer and the naval architect must attack and solve before success can be attained. It may, however, be considered as not at all improbable that those of us who live to the next century may see the Atlantic crossed in less than four days.

THE ENDURANCE OF STEEL RAILS.

From "Iron."

As a great many questions have been asked me as to the comparative durability of iron and steel rails, I have thought that the nineteen years' results on the London and North-Western line of railway, which I have been able to

tabulate, might be of interest to the members of the Iron and Steel Institute, and I beg to submit a diagram to this meeting showing the comparative numbers of tons of iron and steel rails used for relaying purposes on the railway in question, from 1867 to the end of this year, the last year being, of course, the estimated requirements. On the same diagram I have shown the quantity of coal burned yearly in the locomotives, as I take it that this is the only reliable way in which we can arrive at the amount of work done on the line in each year; and, as a check upon the coal consumption, I have also shown on the diagram a line representing the train miles along with the engine miles, and it will be seen at a glance that, while the coal line very closely follows in proportion to the train miles and the engine miles run in each year, the quantity of rails used for renewals has been a constantly decreasing amount since 1877. From 1868 to 1877, we were putting down both iron and steel rails on renewal account. I have shown the iron and steel rails separately in the diagram, and the combined iron and steel in a double line. It will be noticed that in 1868 the quantity of iron and steel rails required for renewals was, roundly, 16,400 tons, and that the largest weight of rails required for renewals was arrived at in 1876, twelve months after which iron rails entirely disappeared —the total number of tons used in that year (1876) being 31,391, while the estimated requirements for this year are only 11,600 tons.

Practically, the whole of our main lines are relaid with steel; and while, in past years, we have been putting down steel rails as fast as iron ones wore out, we are now putting down steel rails as fast as steel rails wear out, except on some of our branches, where iron rails, of course, last a much longer time than on the main line. From what I can see, by watching closely our own line, I believe we have now reached the minimum required for renewals, and that our renewals will rather increase than otherwise, but at a much less rapid ratio than they did up to 1876. The small quantity of rails required for renewals on the London and North-Western Railway, if other companies have re-laid their roads with steel at anything like the same rate, will account in some measure for the depression in the steel rail trade; and, as our steel rails wear out, the quantity of pig-iron required to keep the road going will be represented as nearly as may be by the difference in weight between the rails when put down and when taken up for renewal, plus 7½ per cent. for loss in manufacture. This will also represent very closely the quantity of iron required for the bath in the Siemens furnace for remelting the old steel rails, so that for a considerable period the quantity of iron required on such a line as ours will be much less than it has been during the past period; but, if steel sleepers are found to answer—and I see no reason why they should not—I hope they will, in a great measure fill up the want of orders for steel rails in our various large works.

On our main line, up to the present time, we have put down 45,000 steel sleepers; and, on recently examining those we first put down on the Chester and Holyhead line six years ago, I found that they were in very good order, with no signs of loose rivets, though these sleepers were made with a much less chair base and leverage for the rivets than those we are making now. While on the subject of steel sleepers, I may observe that there has been a good deal of discussion and a number of questions have been asked me as to their tendency to work endwise, especially in curves; but, so far as our experience goes, when properly ballasted up, we do not find this tendency. Our engineer, Mr. Bradford, who has had some of these sleepers down for a considerable time in our South Wales districts on gradients of 1 in 38 and on a curve of 10 chains radius reversing into one of 15 chains radius, finds no tendency to working endwise; and, while going over the road previously referred to in North Wales with some Scottish engineers, I pointed out to them in many cases moss was growing on the end of the sleeper, and on the ballast, without the slightest sign of disturbance. As it may interest the members of the Iron and Steel Institute to see the present form of the steel sleeper and chair, I have ventured to send one for inspection at the meeting.

OBITUARY.

DAVID VAN NOSTRAND.

DIED JUNE 14th, 1886.

The sad duty of announcing the death of the founder and publisher of this Magazine devolves upon us in the preparation of the present issue.

The grief that is born of the breaking of the bonds of a long friendship is but poorly assuaged by the enumeration in type of the virtues that, year by year, made that tie stronger.

Only those who shared the friendship and fully knew the qualities of this truly Christian gentlemen can share the sorrow now. What he was to a large circle of friends is only too keenly felt to require any attempt to express its measure. What he has been to society and the world is a matter of completed history, in which a larger circle of acquaintances will find a sad interest.

DAVID VAN NOSTRAND was born in New York City, in December, 1811. At the age of fifteen he entered the service of John P. Haven, then a bookseller on Broadway, at the corner of John Street. The engagement with Mr. Haven continued till 1834 (eight years), when Mr. VAN NOSTRAND became associated with Mr. William R. Dwight, and established the firm of Van Nostrand & Dwight. The trade of this firm was chiefly in religious books; it continued for three years, the copartnership being dissolved in 1837. During the next ten or twelve years Mr. VAN NOSTRAND devoted much of his time to the study of scientific subjects, giving special attention to works on civil and military engineering. He became an accountant in the employ of Gen. J. G. Barnard, who was then directing the construction of the fortifications near New Orleans. The experiences and associations of his life at New Orleans determined the direction of Mr. VAN NOSTRAND'S later efforts. The acquaintance formed with military engineers, the interest he had cultivated in the subjects that engaged their attention, together with an aptitude for business acquired in his previous experience, all prompted him to begin a trade in scientific books. He began by importing foreign books for the officers of the Army. The business soon assumed unexpectedly large proportions. Importations through his hands were made for various institutions, including the Military Academy at West Point.

In 1850 Mr. VAN NOSTRAND was again established in the book trade in New York. The store at the corner of John Street and Broadway was for nearly twenty years a favorite resort of military men, as well as of students of all departments of science. In a brief historical sketch published in the *Evening Post* in 1875, the following account is given of Mr. VAN NOSTRAND's enterprise:

"At that time (1850) the publication of scientific works as a specialty had not been attempted in this country, although various houses had published fugitive volumes of some value. Encouraged by his growing trade of importation and sale, Mr. VAN NOSTRAND soon ventured to enter upon publishing ventures. In a few years, by persistent and conscientious labor, he built up a business which it is not too strong language to say commanded the admiration of the scientific world, and *Trübner's Literary Record*, of London, while remarking that the United States, although 'prolific in practical applications of science, had been, through obvious causes, somewhat sterile, until a quite recent date, of literary expositions of its works,' admitted that of late years there had been 'a great and rapid development of such works, and with no name is this development more intimately associated than that of Mr. VAN NOSTRAND.' Again this English journal remarked that 'as a gentleman of extensive and varied information, of genial and attractive character, eminent business capacity, and of important achievements in his profession, Mr. VAN NOSTRAND stands prominent among the publishers of the day.' And it well may be added that very few men are so constituted as to be able to wait patiently for the growth of a great business in a field which promises no reward except as the fruits of many long years of toil."

The demand during the Civil War for works relating to military tactics and military engineering naturally stimulated the trade already firmly established at this well-known center. Writers on subjects relating to military science came there to find a publisher, and as Mr. VAN NOSTRAND's scientific knowledge and rare natural power of discrimination were brought to bear upon the offered treatises, it followed that the works published by him became standard authorities, and are now well known everywhere.

The growth of the business at length required more spacious quarters, and in 1869 it was transferred to the present location, in Murray Street. Since the removal the publication of works relating to civil engineering has been an important part of the business, and in this field also Mr. VAN NOSTRAND was foremost in the production of works which soon became standard reference books. During the last two years Mr. VAN NOSTRAND has been an invalid, although not entirely incapacitated for business until the beginning of the present year. Since the 15th of January last he has only been seen by his more intimate friends.

Throughout his busy career Mr. VAN NOSTRAND enjoyed the reputation of possessing rare social qualities. He was a prominent member of both the Union League and the Century Clubs, and in both he was called upon at times to take an active part. He was twice married. His second wife survives him.

The affectionate regard which his sterling qualities had won from a wide circle of his fellow-men, causes deep sorrow for his loss, and will ensure his lasting remembrance.

REPORTS OF ENGINEERING SOCIETIES.

ENGINEERS' CLUB OF PHILADELPHIA.—*Record of Regular Meeting, April 15th*, 1886.

Mr. E. S. Hutchinson read a paper giving a résumé of the Report of the Hon. John Bigelow on the Panama Canal made to the New York Chamber of Commerce.

After stating that the Chambers of Commerce of some European cities as well as that of New York were invited by Mr. De Lesseps to send delegates to assist at the inspection of the Canal in February last, he briefly described the work as follows:

The project contemplates the construction of an open ship-canal, without locks, from Colon, on the Atlantic side of the Isthmus, up the Valley of the Rio Chagres, through the Cordilleras at Culebra, and down the valley of the Rio Grande to the bay near Panama, on the Pacific. Beginning at sea-level at Colon, the canal reaches Matachin, 27 miles, with cuttings varying from 20 to 168 feet; from Matachin to Culebra, 7 miles, the hills are from 100 to 240 feet, while at the latter point the crossing of the Cordilleras, the maximum cut is 320 feet; along the 8 miles to sea-level the decline is rapid. The four miles remaining will be dredged 325 feet wide in Panama Bay to Perico Island. The plan necessarily includes the erection of a breakwater and other extensive harbor improvements at Colon, with a breakwater and jetties, and a tidal-basin at La Boca. Depth of water to be maintained, 27 to 29 feet. In the low lands the widths will be 164 and 72 feet, top and bottom respectively; and in the Cordilleras they will be 102 and 78 feet.

The original plan "Mr. Hutchinson believes" contemplated the construction of "sidings" or passing places every six miles; but the Report mentions only one, 3 miles long, at Tavernilla, 19 miles from Colon.

The most serious obstacles to be overcome are treated of under four heads:

First.—The control of the River Chagres.

Second.—The cut through the Cordilleras at Culebra.

Third.—Keeping the channel open from mouth of the canal at La Boca to near Perico Island.

Fourth.—Securing the labor required at practicable rates.

The Report states that the Chagres, which, with its tributaries, has a normal flow of 450 to 500 cubic feet per second, has recently been known to rise 40 to 50 feet, and discharge 2,550 cubic yards per second; and in 1857 Col. Totten gauged it at 2,093 cubic yards per second; and that the improved channel of the Chagres will carry off 520 cubic yards per second, so that, at a time of maximum flood, 1,573 cubic yards per second will have to be provided for. He mentioned the two plans that have been proposed to get rid of the dangerous surplus; one, the construction of lateral channels, and the other the building of a catch-water basin, large enough to hold a flood or two of the Chagres. Mr. Hutchinson said that within a few days he had been officially advised that both plans are still under consideration by the Technical Commission, though Mr. Bigelow is of the opinion that the "dam" project is most in favor.

He condensed from the Report a description of the "dam;" three-quarters of a mile long, 140 feet high, 1,300 feet wide at base, with exterior slope of 4 to 1. Waste-weir, a 20-feet diameter tunnel, with in-dam orifice 80 to 100 feet *above* bottom of dam. Quantity of material, 9,000,000 cubic yards, with a reservoir capacity of 4,000,000,000 cubic yards. He pointed out the important omission from the Report of the results of the examinations for depth of foundation for the dam, and also of estimate of cost of cost of construction.

He quoted from the Report that the maximum cut at Culebra was 320 feet, and that quantity of material to be removed at this point, and within 1½ miles, was 26,000,000 cubic yards. The Report goes on to state that the Anglo-Dutch Company, which had contracted to remove 915,000 cubic yards per month, has never excavated 131,000 cubic yards per month, that only 1,250,000 cubic yards had been removed up to January 1st, 1886, and that this contract was so modified that from 800,000 to 432,000 cubic yards per month were to be taken out during the next three years, ensuring the completion of the work by July 1st, 1889.

Mr. Hutchinson concluded that from the meagre figures given this would be impossible, and that, if there were no increase in the rate, fifteen years will be required, even if no serious or unforeseen delays occur. He noted the important fact that the Company was to furnish machines and men, and that, having failed in its part of the contract, the contractors were released from all obligations.

He was of opinion that the Report did not show that careful surveys had been made of the Panama terminus, as important questions mentioned elsewhere were not touched upon. "Plans," it states, "are in contemplation for a dam across the Grande, for a dyke 4 miles long, from Gama Point to Naos." No mention is made of the tidal-basin, ¾-mile square, which is deemed indispensable, and has recently been estimated will cost $30,000,000. It went on to state that very little work has been done at this terminus, none within the shore line, and that very extensive repair and construction shops had been erected near the Mangrove Swamps, and that the pestilential exhalations were particularly fatal to skilled labor.

He noted that the Report treated the labor question quite fully. It points out that the native supply was very limited and uncertain, and that agents of the Company were constantly employed in all of the available markets gathering recruits; that the percentage of desertions was heavy; that of the 12,000 men on the rolls, the number is thought to be considerably exaggerated; that unskilled labor, which at the beginning had been 90 cents, was now $1.75 per day; that skilled black-labor ranged from $2.00 to 2.75 per day, while white mechanics received $5.00 gold.

He drew attention to that portion of the Report which states that the American Contracting and Dredging Co. had a contract for excavating 39,250,000 cubic yards of dredgible material from the Port of Colon, from the Main

Canal, and from the Auxiliary Canals, for th improvement of the Rio Chagres, and extending from Colon to Matachin, about 27½ miles. There will be 18⅝ miles of auxiliary canal. Work was begun early in 1884 and has been kept up steadily, there being at present 7 dredges at work. Up to January 31st of the present year this Company had excavated about 7,000,000 cubic yards, the amount for January being 952,000 cubic yards. Mr. Bigelow appears to have no doubt but that this Company will have its work completed by the stipulated time, December 1st, 1877.

Mr. Hutchinson also observed that a list is given of *six* contractors who were to have had contracts amounting to $125,000,000, only one of which, the American Contracting and Dredging Co., is mentioned as having done any work, unless it be that the Anglo-Dutch Co., which is reported as at work on the Culebra cut, is identical with the "Societé de Travaux Publique et Construction Companie," which he is unable to determine.

He considered it desirable to know whether or not any of these companies have thrown up these contracts? What companies are still at work? Where on the line they are located? What amount of work has been done by each?

He quoted from the Report that on January 31st there were 21 dredges and 82 excavators on the work, "with the auxiliary boats, trains and machinery," and added that for further details we must seek elsewhere.

He abstracted the quantities as follows: The total excavation necessary to complete the work was given by the engineers at 157,000,000 cubic yards. To February 1st, 1886, there have been done 18,500,000—11¾ per cent.—leaving 138,500,000 yet to be done—88¼ per cent.

	Million cubic yards.
Total excavation, Jan. 1st, 1886...	17
Total excavation, Sept. 1st, 1884 ..	10
Total excavation, 16 months.....	7

Average per month for 16 months, 425,000 cubic yards.
The work for January, 1886, was 1,400,000 cubic yards.

He remarked that at the latter rate about eight years from February 1st, of this year, would be required to complete the work, were there no other problems than that of excavation to be considered, and that the unknown quantities in the problem were too many for ordinary methods of solution.

Mr. Hutchinson pointed out that, as regards the matter of expenditures Mr. Bigelow adds nothing to M. De Lesseps report of July, 1885; nor is the date of closing of the financial year given. Adopting the figures of this report we have total amount realized to that date: 94¼ million dollars; expenditures, 73½ millions; balance, 20¾ millions dollars. Of the total expenses only 23 million dollars—31 per cent.—were for installing machinery, clearing line and excavation; the remainder were for expenses of organization, supplies and plant. Since that date the company has received 25 million dollars, but what the expenses have been approximately for the past year, more or less, Mr. Bigelow does not inform us.

The paper concluded by remarking that the map accompanying the Chamber of Commerce report was on the small scale of about 46 miles to the foot, and that it appeared to be a copy of one made to show the condition of the work June 1st, 1884—two years ago.

The final conclusion seemed to be that in the interest of engineering it was to be greatly regretted that the inspection and report had not been made by a thoroughly equipped engineer.

Dr. R. P. Robins, introduced by Mr. T. M. Cleemann, read an interesting account of the First Permanent Tramway in America, which was projected by Mr. Thomas Leiper, of Delaware County, Pa., in 1809, for the transportation of stone.

After experimenting in the yard of the Old Bulls Head Tavern, Second street above Popular Lane, Philadelphia, as to the feasability of such a roadway, he advertised in the *Aurora* of September 28th, 1809, as follows: "I wish to contract for the digging part of a railway, from my quarries on Crum Creek to my landing on Ridley; the distance and level has been accurately ascertained by Mr. Reading Howell. The distance is exactly three-fourths of a mile, and an accurate statement of the quantity of digging required may be seen from the plot in my possession, calculated by Mr. Howell. I also wish to contract for the making and laying the rail part of the same, consisting of wood, a specimen of which, as furnished by Messrs. Large and Winpenny, may be seen by applying to them at their manufactory, adjoining the Bull's Head in Second street, in the Northern Liberties. The scantling for the above will be furnished on the ground. I wish to progress in this work immediately."

The work of building and grading was immediately begun, the draft of the road being made by John Thomson, and the railway was finished early in 1810. "The ascents were graded inclined planes, and the superstructure was made of white oak with cross-ties and string-pieces. The cars or trucks were very similar to those now in use, the wheels being made of cast-iron with flanges." "The road continued in active use until 1828, when it was superseded by a canal after the plan made by Mr. Leiper, but not carried into effect until three years after his death, when his son, the Hon. George Gray Leiper, concluded the work which had always been nearest to his father's heart." The site of the old road can still be seen, though it is in ruins, nothing remaining excepting the deep cuts made by the cross-ties. There has been a great deal of discussion of late years with regard to the claim of priority for this road, it having been claimed by various New England writers that an earlier tramway had been built in or near Boston. But as nearly as I can arrive at any conclusion upon the subject, the only road constructed before the building of Mr. Leiper's tramway in Delaware County, was that on the western slope of Beacon Hill, which was designed and executed by Silas Whitney in 1807, and which was about a quarter of a mile in length. "It was used for the transporting of gravel from the top of the

hill down to Charles street, which was being filled up and graded. There were two trains of cars on the railway, so arranged that one train being loaded with gravel would in its descent pull up the empty train. While the full cars were being emptied, the unloaded cars were being filled, and in their descent would haul up the first train, thus doing the work without horses." This road was, however, only temporary, and as the work of grading progressed was gradually removed. It is, however, entitled to a mention as the first work of the kind in America, having been put into active operation at least two years before Mr. Leiper's preliminary and experimental railway in the yard of the Bull's Head Tavern Mr. Leiper's road in Delaware County was, however, the first permanent tramway constructed in this country; the next in point of date "was that laid in Nashua, N. H., in 1825; the third, was the one laid down at Quincy Granite Quarries, in Massachusetts, in 1826–27; and the fourth, the Great Enterprise at Mauch Chunk, Pennsylvania, nine miles in length, to which the former ones are mere child's play."

Dr. Robins' description contains full quotations from various historical authorities, contemporaneous newspapers, etc. A vote of thanks was extended to Dr. Robins for his very entertaining contribution.

Professor L. M. Haupt exhibited an original drawing of Josiah White's, containing designs for Dams No. 3 and 4 on the Lehigh. They were built of round timbers, filled with rip-rap, and were each about 36 feet high. Mr. White is known as the inventor of the earliest form of movable dam, known as the "Bear Trap," which he built at the mouth of the Lehigh, at Easton, in 1818.

Professor Haupt also presented some extracts from a paper on the Philadelphia Traction Co's Lines by Mr. H. R. Stoops, wherein a comparison is made between the cable and horse systems of street railways, favorable to the latter. Remarks were made by Mr. Henry G. Morris on an Electric Motor Car, and by Mr. John T. Boyd on Traction Cables and Machinery.

ENGINEERING NOTES.

A very interesting paper on the irrigation of the oages of Merv and Akhal-Tekke was recently read by M. Pokrovski-Kozel at St. Petersburg, before the Society for the Assistance of Russian Trade and Commerce, Count Ignatieff being in the chair. The lecturer considers the Merv oasis as one of the most fertile spots on the earth. Wheat, rice, and other cereals cultivated by natives for home consumption, yield beautiful crops. The oasis includes about 900,000 acres of cultivable land. But in order to cultivate them it would be necessary to colonise the oasis with civilized pioneers, and to spend about £120,000 on the restoration and extension of the splendid system of canals built up by the Arabs a thousand years ago, and preserved until now in some parts—as, for instance, at the mouth of the river Murhab, about fifty miles from Merv. These canals are 14 ft. deep and 70 ft. wide, and partly used even now by the Merv Turcomans for the irrigation of their fields, though in a primitive manner. The Akhal-Tekke oasis is not so rich as that of Merv, but still it has about 900,000 acres of land suitable for culture. It covers the space of seven miles along the railway line from Mikhailovsk Bay to Khizil Arvat, and could be irrigated by the water from the river Tejen.

Consequent on a survey of the main sewer under the Palace of Westminster, an interim report of the committee on the subject has been presented by Sir Henry Roscoe. "The committee are convinced that the air of the Palace of Westminster is subject to contamination by sewer gas emanating from the low level sewer of the main drainage of the metropolis with which the system of drainage of the palace is in direct connection. Undoubted evidence has been obtained that sewer gas from this source passes into the drainage system of the palace in times of flood, and under the circumstances, owing to the absence of proper ventilation in the low level sewer above referred to and to other causes, the committee are convinced that a complete reconstruction of the main drain under the Houses of Parliament and an entire alteration of the means of discharging the sewage from the palace into the main low level sewer are urgently required for the safety of the members of the Legislature and of the officers residing within the precincts of the palace. The committee therefore beg to recommend to the House that the Board of Works be instructed at once to carry out certain remedial measures which the committee are now prepared to suggest, and which in their opinion will effect the desired result."

A meeting of the Committee of the National Smoke Abatement Institution was held at the Parkes Museum on the 6th inst., Mr. Ernest Hart in the chair. A letter was read from the Home Secretary saying that from correspondence with the commissioners he is satisfied that the police have taken proceedings in all cases of smoke nuisance in which they could properly do so, and exercise due supervision over the steamers on the river, adding that the extension of the area to which the Smoke Nuisance Acts apply is a matter for the consideration of the Legislature. A sub-committee was appointed to correspond further with the Home Secretary, and to urge upon the Government the necessity for the extension of the area embraced by the acts. The secretary reported that the furnace of a steam launch on the Thames at Hampton had been tested by the engineer of the Institution, who in his report stated that during a run of thirteen miles no smoke was visible at the top of the chimney throughout the trip, an improvement of great importance to all owners of launches. Several descriptions of new appliances for smoke abatement were reported and discussed, and it was resolved to publish shortly a selection of the numerous tests of apparatus made by the Institution since the publication of the report on the Smoke Abatement Exhibition in 1881-2.

COMMENTING on the proposals of the Metropolitan Board for dealing with the metropolitan sewage, the London *Lancet* says: "As to the disposal of the effluent, we quite agree that it must be disinfected somehow before it goes into the river. At present the sewage must be treated at the existing outfalls, for the danger of the present system is urgent, and some chemical disinfectant must, therefore, be used. Whether permanganic acid is the best we cannot say; possibly it is. But, although it is inapplicable at Barking and Crossness, land irrigation is a better means of purifying sewage effluent than any chemical disinfection; and, when the sewage goes down to Sea Reach, as we trust it will before many years are past, the final purification will probably be done by the soil, and an almost perfectly pure effluent thrown into the river. The committee are, indeed, so enamored of their permanganate disinfection that they have quite given up the idea of moving from their present outfalls. They say, on the authority of the chemists who advise them, that 'the necessity for land filtration no longer exists, and thus the great objection to the treatment of the sewage and the discharge of the effluent at the present outfalls is overcome. To this we most strongly demur. We are more than ever convinced that the sewage of all London ought not, even after chemical treatment, to be thrown into the river at Barking and Crossness; and we decline to receive on this point the assurance of certainty from chemists who, three years ago, were equally sure that no important injury was done to the river by the raw sewage. We are sorry that the Board persists in this obstinate resistance to the recommendations of the Royal Commission. They have been forced into their present action, after a hard fight, by the pressure of scientific and public opinion, and now, instead of giving in gracefully and obeying the wish of the nation, they contest every inch of ground in their retreat."

STRUCTURES ON COMPRESSIBLE FOUNDATIONS.—The subsoil at Chicago, U. S., is wet clay, and yielding to an extent which has caused serious difficulties on many of the heavier buildings by the unequal settlement. One of the most prominent examples is that of the United States Government building, which was built upon a bed of concrete 3 ft. in thickness; the inequality of the pressure upon the foundations has caused an uneven subsidence and many undesirable consequences have taken place. The concrete foundation has become broken, and cracks in various portions of the masonry, even to distortion of arches, and in two instances stones are reported to have dropped from the decorative work (on April 21) to the jeopardy of persons on the sidewalks around the building. As an example of what can be accomplished by the exercise of engineering skill under similar limiting conditions, the Home Insurance Company's Building, in the same city, is a fire-proof structure of great weight, being 160 ft. in height, and constructed of masonry and iron. The foundation consists of independent piers built of alternate courses of dimension stone and rubble, and the area of the bottom carefully proportioned to a surface of a square foot to each two tons of load to be supported by the pier. In this manner each basement pier and each vertical line of columns rested upon an independent foundation which was loaded to a uniform intensity per square foot. The beams and girders were very securely anchored together at walls and at intersections, and strips of band iron built into the masonry over arches and other places where reinforcement might be desirable. The whole building has subsided $2\frac{1}{2}$ in., but owing to the care in placing loads of uniform intensity of stress upon the foundation, the maximum inequality in settlement has been only $\frac{1}{4}$ in. In our day and generation the wise men are not limited to those who build their house upon a rock, but must include those who make the sand as stable in its resistance as a rock. The original peninsula comprising the city of Boston, U. S., has been distorted into some other geographical form, and more than doubled in area by the filling over the harbor and estuaries by about 16 ft. of gravel over clay and mud forming the bottom. The large buildings constructed upon this "made land" have received the benefit of skilled engineers in regard to the distribution of the loads upon the piling which support the stone foundations, and bid fair to remain permanent without any distortion, but many of the elegant private residences on the Back Bay district of the city, being erected under the sole direction of architects who did not avail themselves of the work of engineers familiar with that special branch, have settled irregularly, and many fine buildings are marred by cracks in walls and ceilings. This criticism does not apply in so great a measure to many of the later buildings where more judicious measures have been introduced to provide for uniform settling. The architects are not alone at fault here, for the abutment piers of a highway bridge over a railway on this district were moved laterally, foundations and all, some twelve years ago by the earth pressure caused by the approaches.

IRON AND STEEL NOTES.

A PAPER was read at the last meeting of the Chemical Society, on "The Influence of Silicon on the Properties of Cast Iron," Part III., by Mr. Thomas Turner. The paper considered in detail the Woolwich Report, "Cast Iron Experiments, 1858." This report included the chemical analyses and mechanical tests of seventy specimens of British cast iron. The author classifies these irons according to the amount of phosphorus present. Some of the more important results are as follows:—(1) Only eight specimens were mentioned as being "too hard to turn;" seven of these contained under 0.9 per cent. of silicon, while the eighth was rich in phosphorus and sulphur, facts strongly supporting the author's conclusion that a softening effect is produced by a suitable proportion of silicon. (2) The six best specimens mentioned in the report contained on an average 1.393 per cent. of silicon, while the author, from his own experiments, recommended

about 1.4 per cent. These and other results support the view that a suitable proportion of silicon is beneficial. (3) When the specimens are classified according to their proportion of phosphorus and arranged in tables in order of silicon present, a gradual improvement is noticed as the silicon increases until a certain point is reached, beyond which point the metal deteriorates in quality. In the discussion on the paper, Professor Unwin noticed the popular prejudice that silicon is a very injurious constituent of cast iron. This prejudice arose a long time ago, apparently from the difficulty experienced in smelting very rich iron ores containing much silica. Thus, the Turkish Government, in 1884, wished to utilize an exceedingly rich ore—magnetic ore containing 12 per cent. of silica—found at Samakoff, but could not smelt it, and the difficulty was attributed to the silica. Again, about 1853, attempts were made to improve cast iron guns, and Mr. Cochrane advised the use of Nova Scotia iron. This was tried at Woolwich, but the Chemical Department there refused to sanction its use on the ground that it contained too much silicon, notwithstanding that Fairbairn's mechanical tests were in its favor.

NEW IRON ENTERPRISE IN SOUTH RUSSIA.—We learn from St. Petersburg that a company has been formed, with a capital of two millions sterling, to work the iron deposits of the Krivoy Rog district, reputed to be the richest in the world. Situated in the Ekaterinoslaff Government, they first became thoroughly known after a systematic survey conducted by the Russian authorities a few years ago. The Ekaterinen Railway was then constructed by the Government to connect the deposits with the coal-fields of the Donetz valley, and since the completion of the line, in 1883, upwards of 3,000 tons of ore have been sent regularly every month to the works of Hughesovka alone, the price, including placing on the railway truck, being 2½ copeckes a pood, or 2s. 8d. the ton. Last year a fresh outlet was opened in Poland, a quantity being sent from Krivoy Rog to several of the iron and steel works in the Vistula region. Yielding 68 per cent. of splendid metal, the ore gave such satisfaction that an international company, favored by the Russian Government, was formed to develop the mines on a large scale. The capital subscribed was 19,500,000 roubles, of which the Warsaw Steel Works have furnished 2,500,000 roubles, Lilpop & Rau 1,500,000 roubles, and the remaining 15,500,000 roubles has been made up by foreign capitalists, including Cockerill & Co., the Grande Société Franco-Italienne des Houilles et Foches à Paris, the Reinische Stahlwerke of Ruhrort, the banker, Surmont, of Aix-la-Chapelle, and Messrs. Ransome & Co , of London. As an encouragement the Russian Government has agreed to give the company an order for 70,000 tons of rails, 30,000 tons of railway material, &c., amounting in value to over a million sterling, of which a considerable amount will be paid by the Government in advance A clause in the agreement also provides for a bounty on steel rails manufactured on the spot. For some time past agents of the syndicate have been completing the arrangements at Ekaterinoslaff for starting the concern, and it is believed that it will be placed on a good working footing by the winter. Another scheme, favored by Krupp, for establishing a gun foundry for the Russian Government in the Krivoy Rog district, has also been discussed during the spring; but the terms asked by the German syndicate were not favorable enough to please the Minister of War. The project, however, has not yet entirely fallen through, and even should no foreign capitalists embark upon the enterprise, it is believed that the Ministry of War itself will establish an arsenal there for supplying weapons for the use of the Black Sea fleet. At present the guns mounted on the coast batteries and men-of-war of the Euxine are manufactured in the Ural Mountains or at St. Petersburg, and the cost of the conveyance over many hundred miles of railway is very heavy. The saving effected by establishing an arsenal in the Krivoy Rog district would thus justify a considerable subsidy.

THE corrosion of steel is a matter of greater importance than some of our experts imagine. It is not only during the first six months that the paint falls off the bottom of steel vessels. I was informed the other day by the principal of a well-known Tyneside anti-fouling paint company, who manufacture both Rahjtens and the International paint, that he has found steel vessels, when even two or three years old, to be almost "bare" when docked; whereas, iron vessels under the same circumstances have still a good "body" of paint left. It is his opinion when two coats of paint are sufficient for an iron vessel, three coats are required for a "steel" one. The fact is "Lloyd's" surveyors cannot see the "mill-scale" properly removed by the painting of the vessel being postponed as long as possible in the case of a vessel being contracted to be delivered in six months—not an unusual occurrence. In a few months the mere action of the atmosphere does little to remove the outer skin of steel, so closely is it combined with the main body of the material. I think in all cases the Government practice of dipping the plates and bars in diluted hydrochloric acid and scrubbing them with steel brushes should be insisted upon.

CHANGES IN IRON DUE TO MAGNETIZATION.—Mr. Shelford Bidwell has been making further experiments on the changes produced by magnetization in the length of iron wires under tension. His results, as recently communicated to the Royal Society, disclose the following facts. An iron wire under tension and subjected to a gradually increasing magnetizing force, is at first elongated, unless the load be great, then it returns to its original length, and finally it contracts. The maximum elongation diminishes as the load increases, according to a law which seems to vary with different qualities of iron. If the ratio of the weight to the sectional area of the wire exceeds a certain limit, the maximum elongation, if any, is so small that the instrument fails to detect it. The retractation due to a given magnetizing force is greater with heavy than with light loads. Both maximum elongation and

neutrality (*i. e.* absence of elongation and retraction) occur with smaller magnetizing currents when the load is heavy than when it is light; retraction, therefore begins at an earlier stage. The phenomena, both of retraction and elongation, are as might have been expected, greater for thin than for thick wires, and for soft than for hard iron.

RAILWAY NOTES.

THE KOLOMNA WORKS.—According to a recently published report, the Kolomna Works in Middle Russia, which are under the control of General Struvé, the eminent bridge builder, have constructed during the last two years upwards of 140 locomotives for the various Russian railways. A few weeks ago the Minister of Ways of Communication, General Possiet, invited tenders for building sixty more. On examining the applications, 27 were assigned to the Nevsky Iron Works at St. Petersburg, belonging to the Russian Mechanical Society; the price arranged per each six-wheel locomotive, of 32 tons, being £75 a ton, or £2,400 apiece. For several years the Nevsky Works have received no Government orders, and hence put in a low tender. The Kolomna Works wanted 800 roubles a ton, or £2,516 apiece for the remaining 33 locomotives, and refused to make them at the same price as the rival St. Petersburg firm; in consequence of which their application was refused. As, however, the Government wants the whole of the 60 with as little delay as possible, and the Nevsky Works cannot do them all in the appointed time, it is believed that the deadlock will result in the Kolomna Works securing its proposed terms. Of late a large number of locomotives have been withdrawn from the Government reserve (several hundred being kept on hand in readiness for any war), in order to equip the Transcaucasian and Transcaspian railways, and the Minister of War is anxious that the complement should be made up afresh this year. We called attention the other day to the fact that the Russian Government, which has been lagging railway construction for several years, has gone ahead again this year, and the additional demand for locomotives this will occasion has led to several firms in Russia expressing their intention of manufacturing them. It will some time, however, before they will seriously compete with the Kolomna and Nevsky establishments.—*Engineering.*

A REMARKABLE instance of the effect of competition by sea and land which at present exists has been brought to our notice within the last few days. The *Railway News* says a contract has just been entered into between the agents of Italian railways for the delivery at Venice of coal shipped at Cardiff and Swansea, free of all charges, at 20s. per ton. This is exactly the price at which the same coal is delivered to the Metropolitan Railway Company in London, the competition between sea-going ships being so severe that the freight is little more than nominal. Another illustration of the effect of competition by sea with our own railways is afforded by the fact that the quantity of coal brought by coasting vessels into London from Welsh ports has increased to such an extent as seriously to curtail the quantity carried by the Great Western to London. For the two months of the current year the decrease in coal carried to London on the Great Western was over 20,000 tons as compared with 1884. There is sea competition also with other ports as well as with London. At the meeting of the Great Western Company Sir Daniel Gooch stated that coal was conveyed from Cardiff to London at 4s per ton, or equal to a railway fare of one farthing per ton per mile—a rate with which the railway companies could not profitably compete.

THE third main division of the report on the railway accidents in the United Kingdom in 1885 deals with accidents to servants, whether of companies or contractors, caused by the traveling of trains or other vehicles on railways. Here we find 438 killed, and 2,036 injured, while the causes of injury are too numerous to be mentioned in detail. The most fatal risk to which the servants are subjected is working on the permanent way, sidings, &c., whereby 107 were killed and 126 injured; while walking, crossing, or standing on the line on duty caused 79 deaths and 108 injuries. Altogether, what with passengers, others of the public, and servants, the total number of persons killed during the year 1885 was 957, against 1,134 of the year before, and of injured 3,467 persons, against 4,100 in 1884. It is satisfactory to note that not only the totals but almost all the items have materially decreased from the year before.

ORDNANCE AND NAVAL.

THE Russian Government are going to test exhaustively the watertight compartments of all their new vessels. They began the other day with a corvette cruiser which was finished last autumn; and although ample notice had been given, so that any little defects might be set right, when the compartments were filled the water gushed out from many places which had been overlooked. Finally, after a great deal of door-adjusting and leak-stopping, the large compartments were proved to be watertight in fact as well as in name. The Russian Admiralty authorities seem determined to take nothing for granted—an example which some other Admiralty authorities nearer home would do well to follow.

AT this year's meetings of the Institution of Naval Architects a somewhat insignificant incident formed a notable feature of the first day's proceedings. A paper had been read descriptive of an instrument invented for the purpose of indicating the strains to which ships are subject at sea, and in the discussion thereupon, Mr. Ramage, a Scotch shipbuilder, bluntly advanced the notion that such subjects were the business of Lloyd's Registry, and not not of shipbuilders; shipbuilders have to carry out Lloyd's rules, and all their energies are required to make shipbuilding pay. Scientific investigation under such circumstances is the business of the framers of the rules, and not

of those who have merely to work them. Shipbuilders present protested against this notion, but only adduced the case of light-draught steamers and other vessels built for exceptional work as affording scope for original design as regards structural strength.

Two steamers for the Caspian have recently been put out of hand by Messrs. Boolds, Shever & Co., of Sunderland. They are named respectively the Cebah and A. H. N. They are fitted to burn crude petroleum, which is carried in an athwartship tank a little less than two feet in length, and thus occupying one frame space and extending the whole breadth and depth of the vessel. This tank is at the fore-end of the boiler room, and beside it there are two fore and aft tanks, one on each side of the boiler-room. Both steamers are built of steel; the Cebah is of dimensions, length, 148 ft.; breadth, 27.1 ft.; depth, 12.2 ft.; gross tonnage, 473. The A. H. V., length, 140 ft.; breadth, 24 ft.; depth, 11.3 ft.; gross tonnage, 370. The steamers will burn coal on their passage out.

The Steel-Wire Gun.—The new experimental 9.2 inch steel-wire gun has just been tried at the Government proof butts, Woolwich Arsenal, with satisfactory results. The War Department have issued orders for the construction of several more guns of the same description. The Government pressure test for the gun was 65 tons to the square inch. The new weapon weighs 25 tons, and is 33 feet long. The steel wire is coiled around the inner tube at the breach, and nearly up to the trunnions, and consists of 78 layers. The wire is made in lengths of 2,400 yards. It is flat, and is put on by a specially designed machine at a pressure of about 40 tons to the square inch. The lengths are joined together by being brazed and riveted together over a length of 15 inches. After the wire has been put on, a steel jacket is shrunk on over it.

Armorplate Trials at Spezia.—Important armorplate trials have just taken place at Spezia. A large chilled-steel plate, manufactured by Gruson, of Buckau, Germany, the greatest thickness of which is 1.75 meter (5 ft. 9 in.), had been fixed in a rock near Castagna Bay, this position being the nearest approach to that it will occupy in the finished turret for wich it is intended. The Italian Government proposes to construct two such turrets for the protection of Spezia harbor. The plate weight 100 tons, and is one of seventeen plates which are to form the turret. Their total weights with the parapet armor, is 2,500 tons. The turrets are to be armed with two breachloaders, firing projectiles of 40 centimeters (15.6 inches) diameter. They are to be proof against guns of the heaviest calibers yet manufactured, with which at present only the Italian navy is armed, but which may be introduced any day in other navies. A 43-centimeter (16.77 inches) gun from the Italian ironclad Lepanto had been placed on the pontoon moored 130 meters from the target. The projectiles used were Krupp chilled steel shells, weighing 1,000 kilogrammes each, the charge of powder being 7½ cwts. of prismatic cocoa powder, of German manufacture, which gives to the projectile an inertia of 15,000 meter-tons. The greatest inertia hitherto attained at the previous Spezia trials had been 13,500 meter-tons, and was employed against steel and compound plates of English and French make. None of them withstood, it will be remembered, a single shot, being either penetrated or broken up. It was the general impression among those who witnessed the trials at Spezia that the Gruson plate would share the same fate, it being assumed that nothing could withstand the force employed. The result proved otherwise. The first shot only caused a slight indentation a few centimeters deep, besides some trifling cracks. The second and third shots were no more effective, and the plate could have stood some more pounding without showing any serious injury. But the officials present considered that the plate had successfully stood the maximum test required. The trials show that turrets protected by such massive armor and armed with powerful guns, are to hold out against any attack, and, by inference, to disable ships attacking them.

BOOK NOTICES

Retaining Walls for Earth. By Malverd A. Howe, C.E. New York: John Wiley & Sons.

The author has employed the method of Prof. Weyrauch, amplifying and applying the original analysis for the benefit of practical engineers.

Readers who do not care to verify the analytical investigation may begin with "Recapitulation of Formulae," on the 46th page. Beyond this, applications to practical cases only claim the attention.

The whole is a neat and compact little treatise on an important engineering subject.

Through the Yellowstone Park on Horseback. By George W. Wingate. 16mo, pp 250. O. Judd Company. Price $1.50.

In the book before us we have the account of a summer's trip made by General Wingate, with his wife and daughter. The party started from Bozeman, with saddle-horses, baggage and tents, and spent nearly a month in riding through Montana to the Yellowstone Park, and then back through Idaho and the Madison Basin—a distance of over four hundred and sixty miles accomplished on horseback.

Graphic descriptions of the wonders of the National Park, experiences at the cattle ranches and with the cowboys, and the various hunting expeditions, are all very interestingly told. A full account of the routes by which the park can be reached, the outfits required both for ladies and gentlemen, the expenses of the trip, an accurate description of the game, and suggestions as to the proper method of hunting it, are all detailed in separate chapters.

The book is admirably written and is filled with illustrations, and is destined to become the authority on travel through that section of the country.

PRACTICAL HYDRAULICS. By P. M. RANDALL. San Francisco: Dewey & Co.

The plan pursued by the author in the preparation of this little hand-book consists:

1st. In demonstrating concisely the principle or principles involved, yet in a manner sufficiently ample to be readily followed by the student, or by any practitioner desiring to refresh his memory or to assure himself of the correctness of the results.

2d. In expressing in words the simplest rule or rules corresponding to the formulae.

3d. In applying the rule or rules thus derived to practical examples, with full and clear explanations.

4th. In providing tables to meet the requirements of practice.

The formulas and tables apply to most of the problems relating to water in motion, whether through pipes, over weirs, in open channels, or through nozzles, and for the purposes of affording power, for mining, for supply, or for irrigation.

ABRIDGEMENT AND DIGEST OF U. S. PATENTS ON UNDERGROUND ELECTRICAL LINES. By JAMES W. SEE, C.E., etc., etc. Published by the author, Hamilton, Ohio.

This book will be a valuable aid to inventors or users of electrical subways.

Wires for electrical conduction for any purpose must eventually be placed below the surface of the ground, at least in our large cities. Whether any one form of conduit will fulfill the many required conditions is yet a question.

In the meantime inventors are busy in devising plans, and are continually adding to the already long list of patented devices for conveying electricity below the surface of the earth.

The claims of this book upon the attention of electrical companies and their engineers are:

1. The abridgement of each patent recites every exhibit of the patent, whether such exhibit be involved in drawing or specification. In this respect the abridgement differ radically from the very unsatisfactory extracts found in the abridgements of English patents.

2. The claims are given in full.

3. Full indexes are provided both chronological and alphabetical.

4. The digest is exhaustive, and thoroughly analytical, ommitting no details. It is thought that this is the first published example of a thoroughly analytical digest of a given class of patents, the digest accompanying the English abridgements being little more than general indexes.

5. The underground line business is assuming vast commercial and legal importance, and the present work becomes indispensable in connection with questions which will arise.

MISCELLANEOUS.

AT the Royal Institution, on the 27th ult., Professor Dewar exhibited the method he employs for the production of solid oxygen. Last year he gave a lecture on liquid air, but although he and other experimenters had made liquid oxygen in small quantity, yet no one had succeeded in getting oxygen into the solid condition. The successful device employed at the Royal Institution depends upan allowing liquid oxygen to expand into a partial vacuum, when the enormous absorption of he..t which accompanies the expansion results in the production of the solid substance. Oxygen in this condition resembles snow in appearance, and has a temperature about 200 deg. Centigrade—360 Fah. below the freezing point of water. It is suggested that a supply of this material will enable chemists to approach the absolute zero of temperature—461 Fah. and to investigate many interesting changes in the physical properties of bodies under the primordial condition of the temperature of space.

A CURIOUS phenomenon has been observed by M. Blondlot, and communicated to the French Academy of Sciences. A disc of platinum and a disc of copper, 0.03 metre in diameter, were fixed vertically in front of each other by help of two platinum stands. The discs were three or four millimetres apart, and both were placed inside a bell jar of porcelain, open below. The apparatus was then heated red-hot for three hours by means of a gas furnace, and although there was no electric current, it was found that the face of the platinum disc was blackened wilh a deposit containing copper and platinum. In short, the copper had crossed from the copper plate to the platinum one. Mr. Blondlot, by repeating the experiment in different gas, found that the nitrogen of the air was the agent in this transport of matter. The nitrogen combines with the copper, and lodges on the platinum, either incorporating itself with the latter or decomposing in contact with it under the influence of its high temperature.

BUNSEN observed the dissociation of steam and carbonic acid by employing small tubes filled with an explosive mixture of these gases, to which suitable pressure gauges were attached. On igniting the gaseous mixture explosion took place, and a high momentary pressure was produced within the tube; from the pressure developed Bunsen calculated the temperature at which the explosion took place, and found that it varied with the mixtures employed. He records the circumstance that only about one-third of the combustible gases took part in the explosion, from which circumstance he concluded that the temperature attained was the limit at which combustion occurred. To prove this, Bunsen allowed the gases sufficient time to cool, after which a second explosion was produced, and even a third explosion when time was allowed for the gases to cool down again. Bunsen's theory seems very plausible, besides which he obtains much higher temperatures for his limits of dissociation than other physicists, so that the figures at which he arrives might be accepted; these are for steam about 2400 deg. C., and for carbonic acid about 3000 deg. C. These temperatures are probably higher than are reached in the arts, as materials used in furnace building would not withstand such temperatures for any length of time; but Mr. F. Siemens calls attention to the

circumstance that if the influence of the inner surfaces of the tubes on the combustion of the gases therein could be removed, the dissociation temperatures arrived at would be found still higher. He thinks that Bunsen's explanation of the cause of the second and third explosions is not quite satisfactory, as it is not the cooling of the gases alone which renders subsequent explosions possible, but also the thorough re-mixture of the gases by diffusion after each explosion.

THE CORROSION OF BOILERS.—MM. Klein and A. Berg have been studying the action of sugars on the corrosion of boilers, and find that sugar in water has an acid reaction on iron, which dissolves it, with a disengagement of hydrogen. The quantity of iron dissolved increases with the proportion of sugar in the water. The salt of iron formed is the acetate. A neutral decoction of malt also corrodes iron with disengagement of hydrogen; but glycerine and mannite are without action on the metal. These results are worthy of note in sugar refineries and places where sugar sometimes finds its way into the boilers by means of the water supplied. The experimenters in question also find that zinc is strongly attacked by sugar; copper, tin, lead, and aluminium are not attacked.

DAMASCENING BY ELECTROLYSIS.—A method of damascening metals by electrolysis has been brought out in France. Two copper plates are put into a bath of sulphate of copper solution, one being connected with the positive pole and the other with the negative pole of a battery. A thin layer of insulating varnish or wax is spread over one of the copper plates, viz., that connected to the positive pole, and the damascened device is etched on it. Now, since copper is by electrolsyis transferred from this plate to the other plate, it follows that only the lines of the drawing can be attacked. A battery of two cells is sufficient for this purpose. When the plate has been bitten to the depth of a millimeter it is removed from the bath and treated with hydrochloric acid, to remove traces of oxide of copper in the lines of the drawing. It is then washed with water and suspended in a bath of nickel and silver, and connected with the negative pole of the battery. The positive pole now consists of a plate of platinum. The silver or nickel deposits wherever the copper has been attacked, and the depressions are soon filled with the foreign metal. The plate is then polished, and looks like one which has been damascened by hand.

SCIENTIFIC EXPERTS AS WITNESSES.—Let us look at this question as it presents itself to men of science, alike to the chemist, the physicist, the mechanician, the geologist, the physician and the microscopist, though certainly not to the astronomer, who is in no danger of being called, as such, to give his testimony. The expert occupies a totally anomalous position in court. Technically he is a mere witness; practically he is something between a witness and an advocate, sharing the responsibilities of both, but without the privileges of the latter. He has to instruct counsel before the trial and to prompt him during its course. But in cross-examination he is the more open to insult because the court does not see clearly how he arrives at his conclusions, and suspects whatever it does not understand. The late Dr. R. Angus Smith complained of being "contemptuously compelled to herd with thieves and scoundrels in a witness-box." He adds: "I have seen barristers speaking to a scientific witness in such a way as to show that to them a witness was always an inferior person." Surely every person who has been present at a technical trial, or has had to appear as an expert in a poisoning, a patent, or an adulteration case, will be able to confirm this from his own observation and experienee.

Now it may, perhaps, be cynically hinted that men of science should be willing to bear all this annoyance for the public good. But is it for the public good? In the first place, not a few of the most eminent men in every department of science distinctly and peremptorily refuse to be mixed up in any affair which may expose them to cross-examination. "I will investigate the matter, if you wish it, and will give you a report for your guidance, but only on the distinct understanding that I am not to enter the witness-box." Such in substance is the decision of not a few men of the highest reputation and the most sterling integrity. Certainly it is not for the interests of justice to render it impossible for such men to give the court the benefit of their knowledge.

Further, the spectacle of two men of standing contradicting or seeming to contradict each other in the interest of their respective clients is a grave scandal. Men of the world are tempted to say that "Science can lay but little claim to certainty, and is rather a mass of doubtful speculations than a body of demonstrable truth." To us, at least, there is nothing more saddening than to read the trial of a notorious poisoner, or the report of a great patent case, especially if taken along with the comments of the press and of society on these occasions.

Here, then, we see that our present mode of dealing with scientific evidence is found on all hands unsatisfactory. The outside public is scandalized; experts are indignant; the bench and the bar share this feeling, but unfortunately are disposed to blame the individual rather than condemn the system.

But we fear that this unanimity of dissatisfaction will vanish as soon as a remedy is seriously proposed. To that, however, we must come unless we are willing to dispense with scientific evidence altogether.

As it seems to us, the expert should be the adviser of the court, no longer acting in the interest of either party. Above all things he must be exempt from cross-examination. His evidence, or rather his conclusions, should be given in writing, and accepted just as are the decisions of the bench on points of law.

Here for the present we must invite the suggestions of our readers, hoping to arrive at some definite result from their collective wisdom.—*Chemical News.*